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BRITISH COLONIAL GEOLOGICAL SURVEYS

PLANNED co-operation in the development of backward areas may well serve modern needs better than unrestricted and often wasteful competition; and this applies particularly in connexion with the mineral development of vast, virtually unexplored regions such as are presented by the continent of Africa. The recent proposal to supplement various Colonial Surveys, particularly in Africa, by American geologists, therefore, deserves careful consideration. In commercial work there has at times been considerable collaboration between geologists of all English-speaking countries, as evidenced during the copper development period in Northern Rhodesia, when millions of subscribed capital were spent on exploration by the companies concerned. The results that they obtained were not, however, generally made public; there is no published map of the special areas which were explored in that large territory.

The present proposal takes for granted that there is, and will be for some time, a serious shortage of geologists in Great Britain and the Dominions. As to the competence of the geologists of the British Commonwealth there can be no doubt; the Dominions and Colonies have been well served indeed by their official Surveys, wherever these exist and where they have been adequately supported. The tempo is now to increase; the support to be given them will be greater.

Many of the problems of geological survey, especially in Africa, are not such as would confront a commercial company. Their study may not lead to immediate results. The work of a geological survey is never finished. There is not merely the question of an intensive search for economic minerals. There are problems of soil erosion, water supply, irrigation, structures, structural materials and hydro-electric problems, the location of land for agricultural needs, and the supply of essential mineral fertilizers. Where minerals are concerned, one is not necessarily considering the immediate exploitation of an economic asset. It is the function of a State Geological Survey to study what may be called 'strategic reserves' of minerals which may not be capable of being put to economic use for some time, or which, for reasons of national policy, it may not be desirable to disperse or even to discuss publicly. This has not infrequently led to conflict of opinion between big business interests, which generally have a relatively narrow outlook, and Government officers, whose duty it is to take a longer and more impartial view.

There are therefore some very practical questions to be answered before embarking on the wholesale recruitment of geologists from the United States for work on Colonial Surveys. Such survey work is a long-term operation for Governments, not short-period research for a company. Will American colleagues working with a Colonial Survey in, say Africa, be content to abide by the rules of an alien service? Will they be ready to lease or lend their national

loyalty? Will they be content to spend long periods of their lives away from their own land, where opportunity and prosperity are still within reach of many? If not, what is to compensate for breaks in continuity of their work? Africa is not to be learned in a day.

Remuneration for such services in the United States is on a much higher scale than is customary in British Colonial or Dominion Geological Surveys. If, in the latter, it had been more generous in the past, there would no doubt be a less obvious shortage of British geologists now. Is it intended to scale up the rate of pay in these services, or are American geologists to receive an additional subsidy from their own or other Government concerned? The position may perhaps be summarized in the reply of at least one Geological Survey to a question regarding aid from the Office of European Economic Co-operation, in which the services of American experts were offered. In effect, it said that the needs and proper programme of that Survey had been painfully apparent for many years; what was required was money to carry them out.

Given adequate funds and sympathetic support, it should not take too long to attract and train men from the Commonwealth and Great Britain to their own geological services. For long-term work, as opposed to an immediate need, this is what is required; and the United States could render great service by helping in the part-training of such men in their own institutions and methods. It would be better to benefit by American generosity in this way than to subsidize American geologists to spend short periods under British administration, perhaps in competition with their fellow-professionals. True co-operation should not, in Kipling's telling phrase, become involved in "the business end of the American invasion".

Industrial competition within the United States has caused, and is still causing, an excessive waste and dispersal of irrecoverable raw materials. Any new discoveries of minerals in other lands should not be drawn into the mill of American productivity before a reasonable brake and limit has been set on the expenditure of such materials. This can only, and must ultimately, be attained by international agreement.

The development of backward areas through the collaboration of American personnel must needs bring with it some preference for American machinery and equipment, and some priority in the allocation to the United States of the raw materials produced. The law "to him that hath shall be given" has a sound philosophical basis, just as the problems of the concentration and dispersal of matter have their counterpart in the second law of thermodynamics. The need of the world to-day is that there should not be too much concentration of any one thing in one particular area, while providing for an adequate supply everywhere. If collaboration has this end in view, it is to be welcomed. Is it invidious to suggest that, concurrently with the development of backward areas, and the relieving of distressed ones, a modest form of rationing of some raw materials should be accepted in the New World?

THE NURTURE OF RESEARCH

The Genius of Industrial Research

By D. H. Killeffer. Pp. ix+263. (New York: Reinhold Publishing Corporation; London: Chapman and Hall, Ltd., 1948.) 27*s.* net.

IN the ever-growing literature dealing with industrial research, comparatively little attention has been given to the mental characteristics needed for research work and whether the training given at universities and technical colleges tends to develop adequately these qualities. In "Glances at Industrial Research", E. R. Weidlein and W. A. Hamor look beyond the walls of the Mellon Institute to the broader professional aspects of industrial research. These problems are briefly considered by Dr. D. W. Hill in "Co-operative Industrial Research"; but were virtually ignored by Sir Frank Heath and A. L. Hethering-ton in "Industrial Research and Development". A more adequate discussion is to be found in "The Future of Industrial Research", issued four years ago by the Standard Oil Development Co.; but apart from highly suggestive passages in Col. L. E. Simon's "German Research in World War II", it is to the scientific and technical periodicals that we must look for the more penetrating discussions of the qualities required in industrial research and the methods by which they are best fostered. Some of the post-war reports, like Dr. Vannevar Bush's "Science—the Endless Frontier" and that of the Barlow Committee on Scientific Staff, or statements from Nuffield College, have made some contribution; but by and large the most useful and fundamental contributions are widely scattered and probably not easily assembled.

Clearly the subject is one that demands some consideration at the present time, when the relative contribution of the technical colleges and of the universities is in debate, and Sir Lawrence Bragg's address on "The Standards of Advanced Studies and Research in Science and Technology", recently published in the *Universities' Quarterly*, will doubtless provoke some discussion on the issues which he clarifies. In the present volume, Mr. Killeffer's treatment is too superficial to make any real contribution to elucidating the qualities which are essential to success in industrial research or the way in which they are best fostered. His book is a treatise on the technique of research and some factors that determine success in industrial research, rather than a discussion of the genius or real nature of research; it indicates to the undergraduate the range of experience he may encounter and some of the techniques he should develop if he enters on a career in industrial research, rather than suggests to a teacher the way in which the inherent abilities of a student may best be nurtured for success in that field.

Mr. Killeffer's diffidence over the term 'genius' indicates that he is half aware that his title is ill-chosen. He does not distinguish very clearly between fundamental, or academic, research and industrial research, and some of his categories of the latter add nothing to his argument. This would be less important if Mr. Killeffer recognized that the creative minds, which are always rare, are required in both academic and industrial research. It is from them that the real advances come, nor are such men and women always the eccentrics that Mr. Killeffer implies. Often they take their place in a team, whether as units or as leaders, quite as harmoniously as their less talented colleagues.

Success in industrial research, in Mr. Killeffer's view, appears to be mainly a matter of methodical thinking, systematic organisation and wise choice of objectives rather than one of inspiration. His prime purpose in writing the book is, by analysing the methods of thought and procedure followed by some outstanding investigators, to display principles that may guide the young investigator to a clearer understanding of the nature of research, and increase his competence. He has accordingly compiled a thoroughly representative collection of first-hand accounts of such work, largely from addresses of Perkin medallists and the like, including such pre-eminent investigators as Langmuir, Genalager, Midgley, Cottrell, Twitchell, Sabatier and R. Williams, and ranging over such varied fields as gas-filled lamps, rubber accelerators, refrigerants, high-octane petrol, synthetic rubber, vitamin B_1, to J. E. Teeple's work on potash recovery and E. Weston's on nickel-plating. The result should have been a source book of industrial research which could take a worthy place beside other volumes in the well-known series published by the McGraw-Hill Book Co.

The accounts, though full, are, however, abridged and are interspersed with comments or explanations by Mr. Killeffer himself. Good as some of this material is, Mr. Killeffer has not left himself the space to deal really effectively with such matters as pilot-plant development, report writing, patents and the evaluation of results, on each of which he has short chapters. While abridgment has robbed the records of something of the effect of full contact with original minds, Mr. Killeffer has not succeeded in making good the defect himself, as is well shown by his chapter, "Thinking out the Problem", in which the whole question of scientific method is dismissed too cursorily; and little awareness of the major contributions to modern thought on that subject is revealed. His own contribution is less than that of F. R. Bichowsky in an earlier book, "Industrial Research".

How great an opportunity has been missed by thus falling between two stools may best be realized by comparing the book not merely with the 'source-book' series already mentioned and such outstanding lectures as F. R. Japp's Kekulé Memorial Lecture, or the Hoffman Memorial Lectures of Lord Playfair, W. H. Perkin or H. E. Armstrong, but also with autobiographies or biographies of the quality of Sir J. J. Thomson's "Recollections and Reflections" or Dr. A. S. Eve's "Rutherford". If Mr. Killeffer had learned the lessons of such works, the present volume might well have taken different shape.

For the rest, the book firmly assaults the popular myth that the processes of scientific and technical advance are mysterious and akin to the methods of the magician. Mr. Killeffer says surprisingly little about the qualities making for successful team-work which have been emphasized in such war-time achievements as radar, atomic energy, penicillin and the like. He recognizes the value of imagination and the ability to see beyond mere surface indications; but his stress is laid, in the main, on those qualities of intelligence and orderly thought and the acquisition of techniques which can be developed by efficient and thorough training as the indispensable minimum for success in industrial research. Without these, or without the help of other workers who possess them, genius itself may be ineffective in industry.

Mr. Killeffer's book may provoke some profitable discussion, and he may have blazed a trail for a more adequate and inspiring source-book. Whether university professors, even in the United States, will find that they can use the book for educational purposes remains to be seen. Mr. Killeffer has at least opened up the subject further; but it is to be hoped that those who follow in his trail will avoid the time-wasting practice of relegating all bibliographical references to the end of the book, instead of placing them where they are normally sought and found by the reader.

R. BRIGHTMAN

DISCUSSION ON ISOTOPES

Institut International de Chimie Solvay: Rapports et discussions sur les isotopes

Septième Conseil de Chimie tenu à l'Université de Bruxelles, du 22 au 27 septembre 1947. Pp. iii+411. (Bruxelles: R. Stoop, 1948.) N.p.

THE first Chemical Congress of the Solvay Institute took place in Brussels in 1922; ensuing Congresses were held at regular intervals of three years until 1937. At each of them, men of science from various countries presented authoritative reviews on topical subjects, and the printed reports of these meetings are still a valuable source of information. It was, therefore, very welcome news when it became known that, after the war-time interruption, the Seventh Chemical Solvay Congress would be arranged in Brussels in the autumn of 1947, under the chairmanship of Prof. P. Karrer (Zurich). Its subject was "Les Isotopes".

The "Reports and Discussions" of the Congress have now been published in a stately volume of more than 400 pages. As was to be expected from the general title chosen, very different sides of the question of isotopes have been under review at Brussels. Prof. F. Joliot (Paris) dealt with "Modes de formation, constitutions et filiations des isotopes, notamment des isotopes artificiels"; Prof. K. T. Bainbridge (Harvard) with "Some Results of Mass-Spectrum Analysis". There followed two papers on the spectroscopy of isotopes; Prof. C. K. Ingold (London) gave a résumé on "Isotopes in the Spectroscopy of Polyatomic Molecules with Special Reference to the Benzene Molecule", and Prof. M. de Hemptinne (Louvain) spoke on "Les isotopes comme moyen d'investigation des spectres de bandes". The next two lecturers paid attention to the chemical technique, Prof. F. A. Paneth (Durham) discussing "The Preparation of Radioactive Tracers", and Prof. A. Langseth (Copenhagen) "The Preparation of Organic Deuterium Compounds". The last three reports were almost exclusively devoted to biological uses of the tracer technique. Prof. G. de Hevesy (Copenhagen) described the "Application of Labelled Phosphorus", Prof. M. Calvin (Berkeley, Cal.) "Radiocarbon and its Application in Chemistry and Biology", and Prof. D. Rittenberg (New York) "The Use of ^{15}N and D for the Study of Chemical Processes in the Living Cell".

This enumeration of the titles will provide some means of gauging the wealth of information contained in the volume, too rich indeed to be briefly abstracted. It is to be added that a Solvay Congress, with its limited number of specialist members and generous allowance of time, provides a particularly good setting for fruitful discussions; in these not only "les membres rapporteurs", the principal speakers, participate, but also the scientific secretaries

of the Solvay Foundation and a dozen or so invited guests. Their joint efforts represent a scientific contribution by no means less valuable than the introductory reports, and Prof. J. Timmermans, the secretary of the scientific committee, and his helpers are to be congratulated especially on the excellent way in which the discussions of 1947 have been seen through the press—no easy task, since the speakers dispersed immediately after the Congress was over, and their scattered notes had to be co-ordinated. That the two official languages of the Congress, French and English, have been printed equally well is perhaps not more than most people are inclined to take for granted in a Brussels publication.

F. A. P.

ROCKET PROPULSION: PAST, PRESENT AND FUTURE

Rockets and Space Travel

The Future of Flight beyond the Stratosphere. By Willy Ley. Pp. viii+374+14 plates. (London: Chapman and Hall, Ltd., 1948.) 18*s*. net.

SINCE the earliest times, man has dreamed of the possibility of leaving this planet to visit the heavenly bodies, and much has been written around this theme, ranging from the satirical and fantastic to the serious and scientific. This book is an encyclopædic survey in a readable and untechnical style of every aspect of the subject.

The author begins by discussing the numerous imaginative stories of journeys to the moon and stars which have been written throughout the ages. Of these, the best known are probably Lucian's "Vera Historia", Kepler's "Somnium", Cyrano de Bergerac's "Voyage dans la Lune", and the novels of Jules Verne and H. G. Wells. The development of rockets is then described. It appears that the earliest mention of rockets which can be traced is contained in a Chinese chronicle which records their use at the siege of Pien-king in A.D. 1232. Soon after that date they were used by the Arabs and in Europe. At the end of the eighteenth century, Hydar Ali and his son Tippoo Sultan successfully employed them against the British in India, and this led to the considerable improvements in rocket design and performance made by Sir William Congreve, and to the establishment of the British Rocket Corps, which distinguished itself on the Continent in the early part of the nineteenth century. From about 1850 until the First World War, rockets as a weapon of war suffered a decline because of the great advances made in artillery.

The tide then turned, and between the two Wars considerable progress in rocket development was made, chiefly in America by Goddard and in Germany by the Verein für Raumschiffahrt, of which the author was vice-president. He gives a most interesting first-hand, though probably not entirely unbiased, account of the fortunes of this Society and of the personalities of its leading members, such as Oberth, Valier and Hohmann, and of others, such as von Opal and Ganswindt. During the recent War, rocket development advanced at an extremely rapid pace, and the book contains chapters in which the chief rockets used by the Americans, British and Germans are described.

For Mr. W. Ley the chief interest of rockets lies in the possibility of their use as a means of escaping from the earth's gravitational field, and he devotes a considerable part of his book to a discussion of space-travel and its problems. The theoretical work of Hohmann and others is described, and numerous diagrams are given in order to illustrate the best and most economical routes to and round the moon and planets, the techniques of landing and other points of astro-navigation. Figures and tables based on various fuels and propellant-mass ratios are quoted, and other problems, such as the effect of gravity and acceleration on the human body, are not neglected. One may not share Mr. Ley's idealism and enthusiasm; but one cannot help admiring the thoroughness with which he investigates, and generally overcomes, all the objections and difficulties which arise.

The volume is exceedingly well documented throughout; perhaps one of its most valuable parts is the extensive bibliography of books and pamphlets, in which are listed about 150 works in English, French, Russian and Italian, ranging from the technical and mathematical to the historical and fictional.

R. A. RANKIN

A MODERN COUNTY FLORA

A Geographical Handbook of the Dorset Flora

By Prof. Ronald Good; including a Chapter on the Soils of Dorset, by K. L. Robinson. Pp. xi+255. (Dorchester: Dorset Natural History and Archæological Society, 1948.) 20*s*.

THE facts of distribution of the flowering plants of Britain are to be found in a long series of county floras which we owe to the flair and enthusiasm of the amateur field botanist. These facts have been the material for wider speculation on and more detailed study of the factors determining the origin and controlling the distribution of the flora. The names of Forbes, Watson, Stapf, Matthews and Salisbury may remind us of the interest of such contributions. Prof. R. Good's book is unique in combining the facts of the typical county flora with a consideration of general and special problems of distribution.

Its predecessor, Mansel-Pleydell's "Flora of Dorsetshire", was published in 1895 and is a good example of its kind. It is interesting to compare it with Prof. Good's "Handbook". In the earlier book, preliminary matter runs to 37 pages and 345 are given to the lists of plants. In the later, the lists of plants take just one half of 255 pages. In both there is a short account of topography and geology. Prof. Good also deals with climate and includes a useful and modern account of soil types and distribution from the pen of K. L. Robinson. Mansel-Pleydell gives some tables comparing the Dorset flora with that of neighbouring regions. Prof. Good has written a lucid account of the origin of various groups of native plants and discusses the status of 470 adventive species.

But the most interesting part of the work, and that which is entirely fresh, is a detailed account of the distribution of plants within the county. There is an introduction on general principles and a brief section on the climatic factor. Climate seems to be the major factor controlling the distribution of about

a hundred species. For most plants, however, edaphic factors are decisive, and the effects of these are dealt with in great detail. Finally, there is a chapter on plants the distribution of which cannot be related to any one dominating factor, and, here, suggestion is accompanied by indications of problems needing further research. The general treatment is throughout pointed by reference to a selection of individual species, and is illustrated by thirty maps. Readers of Prof. Good's "Geography of the Flowering Plants" will recognize in all this the foundation of chapter 13 of that work.

It is clear that this admirable treatment could not have been based on the older type of record. It has been made possible only because the author himself undertook a systematic survey which lasted over nine seasons. In the course of this the occurrence of more than 600 species was recorded for 7,500 stands —an average of seven to nine stands to the square mile. This was a great task, and its effects are seen in the number and spacing of the dots on the distribution maps. From all this has come the present book. It includes the customary plant lists. It is also an original and valuable contribution to plant geography and a signpost to new pastures for the field botanist.

M. SKENE

PROGRESS IN BIOCHEMISTRY

Annual Review of Biochemistry

Edited by J. Murray Luck, Hubert S. Loring and Gordon Mackinney. Vol. 17. Pp. ix+801. (Stanford, Calif.: Annual Reviews, Inc.; London: H. K. Lewis and Co., Ltd., 1948.) 6 dollars.

WHEN the "Annual Review of Biochemistry" first appeared in 1932 it was born almost at its present size, for the first volume, of 724 pages, was little smaller than the latest. Nourished by an increasing diet of publications, the "Review" might have grown alarmingly; but, partly by feeding portions to the newer "Annual Reviews" of physiology and microbiology, the editors have successfully kept it down. The exuberance of biochemistry is such, however, that further procrustean treatment is necessary, and, in 1950, plant material will be removed into a new "Annual Review of Plant Physiology". The wisdom of this decision is perhaps confirmed by the article on plant hormones in the present volume, which contains quite a long section on the control of weeds.

The service rendered by the "Review" to biochemistry is widely appreciated and frequently acknowledged; nevertheless, the present writer cannot forgo the privilege of offering his own grateful thanks. The function of the reviewer of these volumes is limited; he is in danger of preaching to the converted; and he can serve best by trying to attract the attention of uninitiated workers in related fields. He can also indicate what, in his opinion, are the strengths and weaknesses of the volume under review. This volume follows the familiar pattern, with articles on enzymes and on the chemistry and metabolism of the fundamental biochemical substances, with the addition of chapters on more specialized topics. This time the latter include the chemistry of the immunopolysaccharides, X-ray crystallographic studies of compounds of biochemical interest (showing the help given in the elucidation of the structure of penicillin), clinical applications of biochemistry (serum potassium, phosphatases, blood iodine, liver function), metabolism of drugs and toxic substances, clinical aspects of vitamins, biochemistry of carcinogenesis (including the metabolism of carcinogenic substances, factors influencing carcinogenesis and tissue changes in neoplasia), terpenes (especially of the genus *Pinus*), alkaloids, photosynthesis, mineral nutrition of plants, plant hormones, chemistry of penicillin (a 'brief' summary of 41 pages and 216 references), ruminant digestion (tracing the formation and utilization of the volatile fatty acids) and physiological aspects of genetics (featuring the obliging *Neurospora*, viruses and many other organisms, including man). Despite the existence of the "Annual Review of Microbiology", there is an article on bacterial metabolism, a welcome provision for macrobiological chemists wishing to keep in touch. Lack of space prevents the present writer from describing the contents of all these articles, but the titles indicate the importance and fascinating nature of the subjects reviewed.

Reduction in size could be accomplished by stricter editing and control of the scope of each review. Thus, xanthine oxidase is considered in "Nonoxidative Enzymes" as well as in "Biological Oxidations and Reductions"; catalase is discussed in two articles and acetate metabolism in four. "Clinical Aspects of Vitamins" contains matter and formulæ more suitable for a text-book, shows the structural formula of pteroylglutamic acid twice, and contains such statements as, "We must know more of the mechanism of the carcinogenic action of all the factors promoting and retarding the production of tumors", and "In deficiency of vitamin A, certain specific pathologic changes are observed in the epithelial structures. Eventually they give rise to what can be termed vitamin A deficiency."

The subject index is regrettably inadequate. Whereas the author index extends to thirty three-column pages, the subject index consists of only nineteen two-column pages. It occupied fifty pages in 1945. A really comprehensive subject index would add enormously to the value of the work, and if it involved delay in publication, this would be justified. Few textual errors were noted, but "Staphlocoagulase [*sic*], an enzyme from pathogenic streptococci" (p. 38), one misprint on p. 21, two on p. 35 and one on p. 209 may be cited.

Perhaps the greatest problem of the contributors is that of welding brief references to large numbers of papers into readable accounts. This year they have been particularly successful. In addition, combining wise saws with their modern examples, many have made profound remarks designed to clear up current muddled thinking, or have warned against the abuse of modern techniques by the unskilled and the unwary. A discussion of nomenclature is a valuable feature of at least two articles.

Finally, the deft touch is not lacking; we are informed that the blood group enzyme acts not only on Negro erythrocytes but also on those of diseased Caucasians; while the statement that "tocopheryl phosphate removes the calcium that activates the nucleotidase that destroys the coenzyme that catalyzes the oxidation of malate to oxaloacetate", although it may have to be modified slightly in view of later work by Pardee and Potter, brings back happy memories of a childhood spent when life, and biochemistry, were so much simpler.

G. D. GREVILLE

Elsevier's Encyclopædia of Organic Chemistry

Edited by R. Radt. Series 3: Carboisocyclic Condensed Compounds. Vol. 12*B*: Naphthalene; *A*. Compounds containing One Naphthalene Nucleus; Hydrocarbons and Halogen Compounds. Pp. xxx+344+22. (New York and Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948.) To subscribers to whole work, £6 5*s*. 0*d*.

THIS is actually the first part of vol. 12*B*, for it deals only with hydrocarbons of the naphthalene series, their hydrides and the halogen compounds. The much larger number of other derivatives, together with compounds containing more than one naphthalene nucleus, will presumably form the subject of one or more further parts. Series 3 of the 'Encyclopædia' will then be complete, for vols. 12*A*, 13 and 14 have already been issued (for a general description of the whole project, see *Nature*, **161**, 415; 1948). The present volume maintains the standard of excellence of its predecessors; but the rate of progress is behind schedule, and considerable acceleration will be needed if the work is to constitute an up-to-date compendium of organic chemical compounds.

It is stated in the preface that references up to the end of 1944 have been included in this volume, and, as far as the reviewer has been able to determine, the bibliography is complete. More important publications of 1945–47 are also noted. In addition to its attractive lay-out, the book has many pleasing features, including the useful summaries of data given in many tables, the generous use of neatly printed structural formulæ to illustrate reactions, and the sure chemical instinct exemplified by a number of cogent editorial comments. The sections dealing with the more important compounds, such as naphthalene and its monomethyl and monohalogeno derivatives, tetralin, decalin, cadinene and selinene, form readable and informative articles. The volume may be commended to the attention of all organic chemists, whatever their specific fields of interest.

J. W. Cook

Géométrie descriptive à quatre dimensions

Par J. Maurin. Livre 1: Figures du premier degré. Chapitres 1, 2, 3: Définitions, positions, intersections. Pp. 38. (Paris: Libr. Gauthier-Villars, 1948.) 200 francs.

THE reviewer approached this volume in a mood of reflexion as to the proper place in the general scheme of geometrical study for four-dimensional geometry. His own introduction to the concepts of geometry of more than three dimensions was through the excellent volumes of M. Jonffert ("Traité Elémentaire de Géométrie à Quatre Dimensions", 1903; and "Mélanges de Géométrie à Quatre Dimensions", 1907). Though interested, he felt an acute disappointment in the natural limitations therein, which was not overcome until, long after, Bertini's work came to his hands. He therefore read M. J. Maurin's volume with a prejudice that, for the geometer, the general n-dimensional case should be first studied and the four-dimensional enlarged upon later.

M. Maurin, in pointing out the peculiar importance of four-dimensional geometry in the study of some mechanical phenomena, makes a strong case for its independent consideration. The present volume is limited to definitions and the study of the intersections of lines and of planes. His method of presentation is an extension of the method of Monge and is amply illustrated by diagrams which occupy half the volume. The range is too restricted for the reader to obtain any estimate of the ultimate value of the method of approach, but the author suggests that interesting developments will be found in later volumes, which are much looked forward to. In view of his special plea for the importance of four-dimensional study it would have been well if a little more space had been devoted in the introductory pages to initial ideas; the first few paragraphs are so condensed that the student with no previous knowledge will find them insufficient for his understanding.

H. Gwynedd Green

X-Ray Optics

The Diffraction of X-Rays by Finite and Imperfect Crystals. By A. J. C. Wilson. (Methuen's Monographs on Physical Subjects.) Pp. vii+127. (London: Methuen and Co., Ltd., 1949.) 6*s*. net.

THIS work fully maintains the standard that is now expected from this well-known series. The author has set himself well-defined limits, has stated these clearly in the preface, and has treated the subject-matter within these limits in a workmanlike fashion. It is unfortunate, therefore, that the main title of the book is deceptive, and that it has to be qualified by a sub-title; a book on X-ray optics should have image formation as its main content; but this subject is not dealt with at all in this book. The author justifies this omission on the grounds that crystal-structure determination is closer to chemistry than to physics. The reviewer does not agree with this; the results are certainly of chemical interest, but the *methods* are essentially physical.

Apart from the title, however, there is little that one can criticize. The author has collected together a variety of problems in the diffraction of X-rays by imperfect crystals and has shown how they can be treated mathematically. He has taken pains to introduce the subject gradually—perhaps too gradually for those who are able to master the later chapters—and therefore almost everybody will gain something from the book. In addition, the style is pleasingly informal. The book can be confidently recommended to those interested in X-ray diffraction, particularly to those concerned with the finer details.

The Way of a Plant in Field and Garden

By Gareth H. Browning. Pp. x+141+7 plates. (London: Rockliff Publishing Corporation, Ltd., 1949.) 8*s*. 6*d*. net.

MR. GARETH BROWNING, author of several works on popular natural history, has written another little book which seeks to explain the functions of the flowering plant in such a way that it will be scientifically correct and also intelligible to the general reader. From both points of view he has achieved his object, and his book will be of value to the non-specialist interested in agriculture, horticulture or the natural history of plants. Whether the book will appeal to them sufficiently to make them want to buy it is another matter. Authors of successful books on popular subjects to-day realize the value of visual appeal and, generally speaking, have set a high standard in the range and quality of the paintings, photographs and diagrams of the illustrations to their books. The seven coloured photographs and thirteen line-drawings in "The Way of a Plant" are not enough, neither are they of a high standard.

T. H. H.

EDWARD JENNER, BENJAMIN WATERHOUSE, AND THE INTRODUCTION OF VACCINATION INTO THE UNITED STATES

By Dr. E. ASHWORTH UNDERWOOD
The Wellcome Historical Medical Museum

THE two-hundredth anniversary of the birth of Edward Jenner, the discoverer of vaccination, was celebrated on Tuesday, May 17, by functions in many places, but notably at Berkeley, Glos, his birthplace, at the Royal College of Physicians, the Royal College of Surgeons—where Sir Edward Mellanby gave an address—and the Wellcome Historical Medical Museum. Much of what can be said—within the limits of an article—on Jenner's life and work has already been said. This present tribute to a man to whom mankind owes so much will therefore be concerned mainly with those aspects of his life and work which have some bearing on the introduction of vaccination into the United States. Although these events are well known in America, the important work which Benjamin Waterhouse did is not fully appreciated in Britain. In this brief article, it is impossible to avoid ground which has already been covered in certain American journals.

Jenner's Early Life

Edward Jenner was born at the Old Vicarage, Berkeley, on May 17, 1749. His father was vicar of Berkeley, and his elder brother Stephen was also in Holy Orders. Edward was apprenticed to a surgeon at Sodbury when he was thirteen, and for the next eight years he had excellent practical experience. When he was twenty-one he became a resident house-pupil in the London house of John Hunter, and during the two years while he was a pupil, a friendship developed between Jenner and Hunter, twenty-one years his senior. In 1772, when Jenner seemed set for a London career, he succumbed to the lure of his native county—he was essentially a countryman—and returned to practise at Berkeley. There, with brief interludes, he remained the rest of his life. Until Hunter's death in 1793, he and Jenner corresponded regularly, and the elder man constantly encouraged the younger in natural history pursuits, and as constantly requested specimens of the most varied kind. It was early in this correspondence that Hunter gave Jenner the famous advice: "But why think, why not try the experiment?"[1]. This encouragement resulted ultimately in Jenner's admirable paper on the habits of the cuckoo[2], in which he described the method whereby the young cuckoo clears the nest of its foster-parent. Often discredited, this work has been completely vindicated in recent years.

Jenner and the Concept of Vaccination

Baron says[3]—probably with truth—that the idea that cowpox could confer protection against subsequent infection with smallpox first entered Jenner's mind when, as an apprentice at Sodbury, he heard a young countrywoman say when smallpox was mentioned in her presence: "I cannot take that disease, for I have had cow-pox". He certainly reflected on this statement, and in later years he was impressed by the fact that, when he was inoculating smallpox matter for 'protective' purposes, the operation was unsuccessful in those who had previously had cowpox. The principle of inoculation was that the individual was given what was hoped would be a mild attack at a convenient time, and was thereby protected against a natural attack of the disease, which was often severe and loathsome, and fatal in as many as 50 per cent of the cases. This practice was introduced by Timoni (1714) and Pylarini (1715); but the effective agent in popularizing it was Lady Mary Wortley Montagu (1721). By Jenner's day it was a common practice. It was a bad method, since the resulting disease was often not mild, and sometimes caused death. Further, each inoculated person acted as a focus for the dissemination of true smallpox.

To return to Jenner. He was well aware of the fact that cowpox was found only in certain counties in Britain, and moreover was present only at intervals. Smallpox, on the other hand, was possibly worst in London, where at that time it was causing a rough average of two thousand deaths annually. Natural cowpox would therefore not be very effective in preventing smallpox in a crowded city where only a few people handled cows. On the other hand, if he could show that cowpox occurring naturally in man could then be transmitted from one individual to another without losing its power to protect against smallpox, there would be no limit to the number of individuals who could be protected. These were the two propositions which Jenner set out to test experimentally, and the results were set out in his "Inquiry into the Causes and Effects of the Variolæ Vaccinæ, a disease discovered in some of the Western Counties of England . . . and known by the name of the Cow Pox". This small book was published in the summer of 1798, and two further editions were published later. I have given elsewhere[4] a critical statement of the genesis and reasoning of this book, and it must suffice to state here that, though the cases were few, they did form a *prima facie* case.

Events Following Publication

After publication of the "Inquiry", Jenner went to London to seek volunteers for vaccination; but though he stayed there three months he was unsuccessful. Vaccination in London was popularized by Henry Cline, who started with some lymph which Jenner gave him; by Dr. Woodville, of the Smallpox Hospital, and by Dr. George Pearson. It was soon evident that Woodville and Pearson were planning to take the whole credit for the introduction of vaccination; but Jenner's equanimity enabled him to stem the tide. Vaccination was soon widely practised in Britain. Various contretemps occurred, many of which Jenner had foreseen. In May 1799, De Carro introduced vaccination to Vienna[5], and German cities soon followed. In January 1800, the "Inquiry" was translated into French, and was published in Paris soon afterwards[6]. It should be remembered that Britain was then at war with France—which explains the delay. Vaccination spread from France to Spain during the latter part of 1800, and it was soon widely practised in most European countries. From the very time when the

announcement of the discovery was first made, the emphasis was always on *free* vaccination. By the end of 1799 various attempts had been made in provincial cities to form "institutions for gratuitous inoculation", that is, vaccination[7]. In London, Pearson formed a vaccine board, which was to carry out free vaccination. As early as September 1798, Jenner had been pressed by Henry Cline and Sir Walter Farquhar to take a house in Grosvenor Square; they promised him £10,000 per annum. He declined, and in a letter to a friend at this time he wrote:

"Shall I, who even in the morning of my days sought the lowly and sequestered paths of life, the valley, and not the mountain; shall I, now my evening is fast approaching, hold myself up as an object for fortune and for fame?—Admitting it as a certainty that I obtain both, what stock should I add to my little fund of happiness?

"My fortune, with what flows in from my profession, is sufficient to gratify my wishes; indeed so limited is my ambition and that of my nearest connexions, that were I precluded from future practice I should be enabled to obtain all I want. And as for fame what is it? a gilded butt, for ever pierced with the arrows of malignancy"[8].

Introduction of Vaccination to the United States

The fact that vaccination was introduced into the United States by Benjamin Waterhouse is stated briefly in all works dealing with the history of vaccination. Baron, writing in 1838, had quite a lot to say about the event. But though the work of Waterhouse was recognized in most parts of America and abroad, in Boston, where he spent all his active life, he was half-forgotten. This was due to the fact that he was never popular with the other members of the profession at Boston and Harvard. He was a Quaker, an active controversialist, an opponent of the political party in power, and at heart a scientific man who had perforce to practise medicine. It was the publication of the paper "Waterhouse, the Jenner of America", by William H. Welch, in 1885, that really placed him in his proper place[9]. Welch gives all the relative facts, but for a true appreciation of the various difficulties encountered we must go to Waterhouse's own publications on vaccination[10,11]. These, like other early vaccination tracts, are now very rare. First let us look at the man.

Dr. Benjamin Waterhouse

Benjamin Waterhouse was born in Newport, Rhode Island, on March 4, 1754. His mother was a niece of John Fothergill, the famous London physician, and in due course Waterhouse studied under Fothergill. He also took courses at Edinburgh and for four years at Leyden, where he graduated in 1781. After assisting his uncle for a time he returned to practise in Newport, Rhode Island, in June 1782. A medical college was then being inaugurated in association with Harvard College, and Waterhouse was invited to take the chair of the theory and practice of medicine. He was the first in America to give a systematic course of instruction in botany and mineralogy, and he founded the Botanical Gardens at Cambridge, Mass., and the collection of mineralogy at Harvard. Despite these advances, his unpopularity and the acrimonious disputes in which he was involved led to him being deprived of his chair in 1812. He died at Cambridge on October 2, 1846.

Benjamin Waterhouse
From Lettsom's "Hints", vol. 3 (1801)

In the beginning of 1799 Waterhouse received from his friend John Coakley Lettsom in London a copy of Jenner's "Inquiry". Waterhouse says that the disease cowpox was totally unknown in that part of the world. He goes on: "On perusing this work, I was struck with the unspeakable advantages that might accrue to this country, and indeed to the human race at large, from the discovery of a mild distemper that would ever after secure the constitution from that terrible scourge, the small-pox"[10]. Soon after, he received a copy of George Pearson's book on vaccination, and he sent to the *Columbian Centinel* of March 12, 1799, a short account of the method under the title "Something Curious in the Medical Line". He reprints this article in the first part of his "Prospect"[10]. He says that he wrote the article to direct the attention of the dairy farmers to the existence of cowpox. A few weeks later he showed Jenner's book at a meeting of the American Academy of Arts and Sciences. The president of the United States, John Adams, was at the meeting and was much interested[12]. Waterhouse conceived it his duty, considering his official position in the University, to give the matter a trial. After several unsuccessful attempts to obtain cowpox matter from England, Waterhouse succeeded in getting some from Dr. Haygarth of Bath—this lymph having come from Jenner's stock. With this matter he vaccinated seven members of his own family, and thereafter he invited Dr. Aspinwall, the physician to the smallpox hospital, to inoculate them with smallpox virus. The test was quite successful, though all were not inoculated. He then says that he had laboured to collect all the evidence on this subject, and to lay it "before the American public, in so plain and simple a manner, as to require no other preparation for its admission than common sense and an unprejudiced mind"[10]. Waterhouse had obviously been receiving requests for vaccine matter from professional men, and in an

Lyman Spalding
From J. A. Spalding

appendix to the first part of his "Prospect", which is dated from Cambridge, August 18, 1800, he writes: "To those of the faculty who have applied to him by letter *to supply them with matter for inoculation* —He would just observe, that as he has taken much pains in this business, run no small risk of reputation, as well as of personal feelings, there are *few* he trusts, that will wonder he is anxious to have the matter under his own eye until the practice is more firmly established by the public opinion. Some unsuccessful cases at the beginning, deprived Scotland of the blessing of inoculation for the small-pox for more than 20 years. He hopes this idea will operate as an excuse at present, even in the minds of his *ci-devant* pupils.

"Dr. Waterhouse informs those who have applied to him out of Cambridge, to inoculate their families, that he declined it only until the disorder had gone fairly through his own family, and until some of them had been inoculated by Dr. Aspinwall, and otherwise exposed to the small-pox. But having now confirmed his assertion, *that the kine-pox protects the constitution from the infection of the small-pox*, by a fair experiment, he is ready to attend them whenever they choose. Those who live in Boston, may rest assured, that from the proximity of his residence to the capital, he shall make such arrangements as to be able to attend them as punctually as if he resided there."

In Part 2 of his "Prospect", published in 1802, Waterhouse says that by September 1, 1800, he had vaccinated about fifty persons. The public attention was now fairly excited, and he was embarrassed by numerous applications for vaccine. But soon everybody believed that the operation of arm-to-arm vaccination was a simple matter and could be performed by anyone. Gone was Jenner's advice on the correct stage of the vesicle for the withdrawal of lymph; gone also were Waterhouse's own injunctions. Women and children were encouraged to inoculate each other, and by the autumn of 1800 there was a traffic in cowpox matter by non-professional persons. Waterhouse says: "I have known the shirt sleeve of a patient, stiff with the purulent discharge from a foul ulcer, made so by unskillful management, and full three weeks after vaccination, and in which there could have been none of the specific virus; I have known this cut up into small strips, and sold about the country as genuine kine-pock matter, and coming directly from me. Several hundred people were inoculated with this caustic morbid poison, which produced great inflammation, sickness, fever, and in several cases *eruptions*, with a greater disturbance of the system than what occurs in the true disease. It is worthy of remark, that I could not influence these people to believe that they had *not* passed through the true disease, and that they were *not* secure from the small-pox."

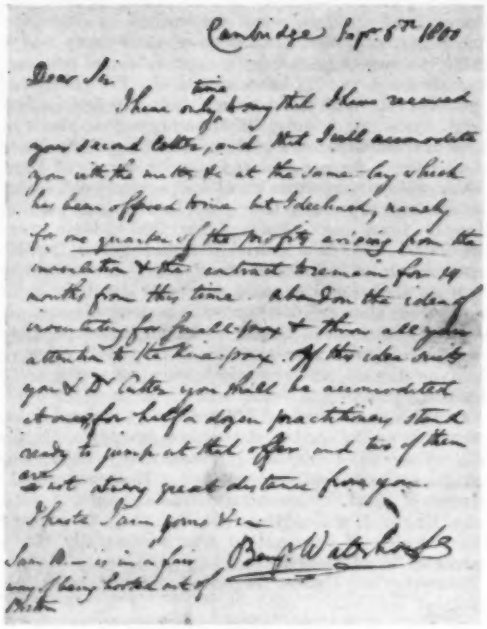
At Marblehead a vessel arrived from London having aboard a sailor supposed to have cowpox. Matter was taken from him and distributed widely, and many cases of smallpox resulted. However, in the winter of 1801, Waterhouse obtained new matter from England, and advice from Jenner. As a result, the practice of vaccination was successfully established in Massachusetts.

Waterhouse always regarded Jenner with the greatest admiration and respect. It is worthy of note that, in a letter which he wrote to Jenner on April 24, 1801[13], Waterhouse tells of the transmission of smallpox to cows. At one of the inoculation hospitals—where smallpox matter was used—several persons drove their cows to the neighbourhood, so that they could be milked for their families in the hospital. The milking was done by persons in all stages of the smallpox. The cows developed eruptions on their teats and udders, "so like the small-pox pustule, that every one in the hospital, as well as the physician who told me, declared the cows had the small-pox".

Dr. Lyman Spalding

During the early stages, when Waterhouse was receiving many requests for vaccine lymph, he had correspondence with Dr. Lyman Spalding. Most of this correspondence was published in a biography of Spalding by his grandson[14]. Some of it has also been re-published, notably by Leikind[15]. As the originals of two of these letters are now in the Wellcome Collection, and as this story is virtually unknown in Britain, they are re-printed here in facsimile. They illustrate an amusing incident, which was perhaps unavoidable in the social atmosphere of that time, but which is not at all representative of Waterhouse's character and of the great work which he did for vaccination and American medicine.

Lyman Spalding[16,17], the originator of the United States Pharmacopœia, was born in Cornish, New Hampshire, on June 5, 1775. As a boy he attracted the attention of Dr. Nathan Smith, who finally had him sent to the Harvard Medical School. He was a pupil of Waterhouse, and graduated in 1797. He assisted Smith in the foundation of the Dartmouth Medical School in 1797, and he gave the first course of chemical lectures when the school was opened. He then studied medicine and practised at Portsmouth, New Hampshire. There he printed yearly bills of mortality, and sent them to the leaders of the profession in America and in Europe. He was well known



(a) Letter from Waterhouse to Spalding
Original in the Wellcome Historical Medical Museum (39826·1)

as an anatomist and a surgeon, and he was the first in America to inject the lymphatics. In 1810 he was invited to lecture at the Fairfield Medical School in New York State, and in 1813 he became president of the institution. He now established himself in New York City. Even in 1815 he had the idea of a national pharmacopœia; but it was not until 1820 that he was able to reconcile conflicting interests, and the first edition was published in that year. Spalding died as the result of an accident on October 21, 1821.

Letters in the Wellcome Collection

The two letters to which I referred are reproduced here in facsimile. The texts are as follows:

(a) Waterhouse to Lyman Spalding, at Portsmouth (W.H.M.M. (39826·1)):

"Cambridge, Sept. 6th, 1800.

Dear Sir,

I have only time to say that I have received your second letter, and that I will accomodate you with the matter &c at the same lay which has been offered to me but I declined, namely for *one quarter of the profits arising from* the inoculation & the contract to remain for 14 months from this time. Abandon the idea of inoculating for small-pox & throw all your attention to the kine-pox. If this idea suits you & Dr Cutter you shall be accomodated at once, for half a dozen practitioners stand ready to jump at that offer and two of them are not at very great distance from you.

I haste I am Yours &c
Benjn. Waterhouse

Sam B.— is in a fair way of being hooted out of Boston."

(b) Spalding to Waterhouse (W.H.M.M. (39826·2)):

"Portsmouth 10 Sept 1800

The terms are accepted, & I promise that you shall have one quarter part of the neat profit arising from my inoculation with the kine pox for the space of 14 months, provided it be not made public before that term expires—& then the contract to remain in full force only to the time of its becoming public: however on your part it is expected, that the like priviledge will not be granted to others in my vicinity.

L. Spalding.

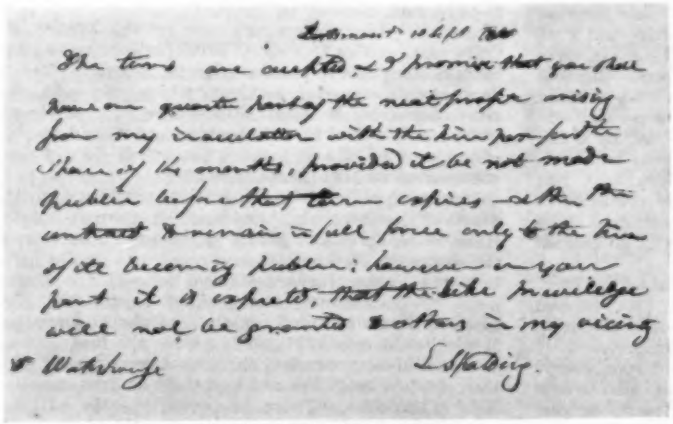
Dr. Waterhouse"

Additional Material of J. A. Spalding

James Alfred Spalding was the grandson of Lyman Spalding. He was born in 1842. In 1908 he was given Lyman Spalding's papers to look over, and this biography[14] was published in 1916. This book is very little known and there is probably only one copy in the libraries of London. It is not in the British Museum.

Since the biographer had all the papers, he was able to give much additional information on this interesting subject. The correspondence opened on August 25, 1800, when Spalding heard that Waterhouse had vaccinated his household successfully, and he asked for a supply of lymph. Waterhouse replied *on September* 6, 1800, advising Spalding not to vaccinate in a smallpox hospital. It is not clear whether Spalding had meanwhile written again; but, though there is confusion of dates, this must be the case. Waterhouse then wrote the letter which is reproduced here as letter (*a*). J. A. Spalding says that Lyman's reply had not been preserved; but that there was a copy of it. This must be letter (*b*). It is apparently written and signed by Lyman Spalding, and bears the name of Waterhouse on the reverse—as if it had been given to some friend to deliver.

The Dr. Cutter mentioned in letter (*a*) may have been either the father or the son—both being well-known physicians at Portsmouth. "Sam B." was Sam Brown, who had been a friend of Lyman Spalding



(b) Letter from Spalding to Waterhouse
Original in the Wellcome Historical Medical Museum (39826·2)

at Harvard. Lyman Spalding was the youngest physician in the town, and Waterhouse was evidently disinclined to give his cowpox matter to him alone, to the exclusion of an older physician.

Sam Brown had been following up the patients vaccinated by Waterhouse, to see whether the scabs could not be used to vaccinate his own patients. In his next letter—September 12—Waterhouse says that the time factor proposed is too vague, as Sam Brown may steal the method within a month. He therefore proposed that the term should be for twelve months. "You, however, will have *got the start of all the others*, so much that no one can rival you entirely; besides it will fix you in business. My fee is Five Dollars." Each man stuck to his guns—Spalding for monopoly, Waterhouse for Cutter and a higher figure. On September 18, Waterhouse informed Spalding that he had divided New Hampshire into three vaccination districts, with the exception of certain specified towns.

At this time the method was generally in use of transmitting the vaccine matter on pieces of linen thread, and it was for one of these threads that the bargaining was taking place. From a bond which existed when the book was written, it would appear that Waterhouse sometimes let one of these go for 150 dollars. On September 25, however, Waterhouse did send Spalding a thread; but he did so under protest that older men should have been included in partnership, and declared that he hoped that Lyman would take them in with him, so that vaccination could be extended everywhere as rapidly as possible. Spalding acted at once on receipt of the thread, and in one day vaccinated thirty persons. Shortly after this it was found that the virus could be passed from arm to arm, and linen threads and bonds immediately became valueless.

The two letters reproduced here in facsimile were presented to the Wellcome Historical Museum by Sir William Osler on January 31, 1919, eleven months before his death. In the note accompanying the two documents, Osler said that from them it would be seen that "they tried to make it a commercial affair; this I did not know". Item 6814 in the "Bibliotheca Osleriana"—the printed catalogue of Osler's library—is this biography by J. A. Spalding, and it seems strange that Osler had not at least glanced at it.

The manner in which these two letters came into Osler's hands is unknown. One can only assume that these two items were given to someone from the Spalding collection. If they had been given to Osler direct by the owner of that collection, Osler would have known a lot about the subject. Further, if the Spalding collection had been broken up, he would almost certainly have heard the fact. Would he not also have picked up many more letters, for was he not "William the Fowler, Guillaume l'Oiseleur", who kept all that was taken in his net ?[17]. It would be of interest to know what has happened to the Spalding letters as a whole.

On March 4, 1801, Jenner wrote his first letter to Waterhouse. It is a long letter full of sound advice, and is published by Waterhouse in full in the second part of his "Prospect". In 1802 Waterhouse was for a time forced to hire volunteers in Cambridge to keep up the strain of fresh virus. The correspondence with Jenner continued. This second part of the "Prospect" was criticized by the Massachusetts Medical Society as being obscure and not worth looking at. But he was much helped by British friends, and the book sold fairly well in the South. Meanwhile, vaccination was spreading to other parts of the United States, and this is too large a subject to deal with here.

A Third Letter in the Wellcome Collection

This is half of a four-page letter on foolscap. It has not, so far as I know, been published previously. The text is as follows:

(*c*) Waterhouse to an unknown correspondent (W.H.M.M. (95443)):

". . . I have just finished reading the different state papers relative to the armed neutrality. Partial as I am to Old England, I must confess that I think these northern nations have truth, and reason on their side. I do not see at present what arguments Great Britain can bring to bear against them, unless it be the *ultima ratio regum*, and that may not be a conclusive one, while this confederacy is so strong, aided as it is, by so potent a monarch as that of Prussia. France will doubtless try to bring us into this combination, and probably will expect to accomplish her views under *Jefferson*, which she would have despaired of under *Adams*. She will nevertheless fail in her expectations. The merchantile interest has great weight in these States, and our Executive cannot bend it to its wishes.

"Jefferson is a prudent man. He sets out with conciliating parties. He says 'we are all federalists, all republicans', and has delivered *sentiments* universally approved & admired. His speech is doubtless in London before this time. As a fine composition the man of taste will read it with pleasure. Jefferson is besides a man of mild & pleasing manners, naturally disposed to complaisancy, whereas Adams was a perfect *John Bull*, and accompanied his honesty with a roughness that in some cases narrowed his way to usefulness. You will find President Jefferson's speech in one of the papers in the box containing the cheese.

"I have just written to our Medical Society to ask their advice respecting the best method of diffusing the vaccine matter through our Land. I have also proposed to them a plan for a *Vaccine Institution* for inoculating the poor gratis. I wish I could obtain a copy of the rules, directions &c &c of the Vaccine Institution in London. Had I thought of it earlier, I would have requested a copy of it from Dr. Pearson.

"I am sorry, very sorry to hear that Jenner & Woodville even glance in their respective works at anything like personalities. We have honored & esteemed these celebrated men for their beneficent productions, and shall lament if in place of affording us more good & wholesome fruit, they should produce thorns. For more than a year I have held up these two eminent physicians to the *American public*, as men who could scarcely err. On that account I feel doubly mortified, that two, out of the three oracles which I have constantly appealed to, should clash in opinion. We feel interested in those we admire.

"I think I have already said that the matter which was on thread between two plates of glass, envelloped in a note to you signed '*Jno. Cn. Wachsel*', produced the disease to *perfection* in every patient I inoculated with it.

"I propose to write again in about a month by a Mr. Nancrede, one of our first booksellers. Till then adieu! and remember that I am your affectionate friend

B. Waterhouse"

April 14th, 1801

In this letter the direct comparison of Jefferson with Adams is interesting. Waterhouse had had

contacts with both these presidents of the United States. When he was at Leyden, John Adams was sent by Congress to Holland to court an alliance, but he was forced to remain a year before he was publicly recognized as the American minister. During this time Adams, his two sons, his secretary, and Waterhouse formed one family[9]. Twenty years later he sent Jefferson a copy of his pamphlet on cow-pox—that is, the first part of his "Prospect"—and on December 25, 1800, the President sent him a very cordial letter of thanks, which Waterhouse published in his second part[11]. Six months later Waterhouse wrote a long letter to Jefferson, giving instructions how to perform vaccination, enclosing infected thread and books and drawings. In this letter he also suggested that the President should arrange for an experiment to be made by "some cautious discerning person, perhaps his own family physician. A series of experiments may be directly instituted by him." Three unsuccessful attempts were made to vaccinate the President's family with three separate batches of "virus". A fourth attempt was successful. The President was attentive to all applications made to him for cowpox matter, and he it was who was thus responsible for introducing vaccination into the District of Columbia, Pennsylvania, Maryland and Virginia.

The reference to Woodville recalls the fact that he was one of the first in London—after Cline—to practise vaccination. But he performed the operation in the Smallpox Hospital, and three days after vaccinating he inoculated the patients with smallpox matter. The result was that the "cowpox matter" from the pustule was really a combination of cowpox matter and smallpox matter. This material, distributed far and wide, caused many smallpox rashes. Jenner had considerable difficulty in saving the situation.

The great esteem in which Waterhouse held Jenner is seen in this letter, and is even more fully expressed in the second part of his "Prospect"[12].

Back to Jenner

If much of this article has been devoted to persons other than Jenner he would not have minded—even on his birthday. In the mass of correspondence which has been preserved, dating from the time of publication of the "Inquiry", we get a very good picture of the man. During the early stages of the movement to promote vaccination he was the oracle to whom everyone turned for guidance, and loaded with honours as he was, he never lost his head. Even his erroneous views—and that there must have been erroneous views at that period when immunology was still unborn is obvious—were often blessings which kept some semblance of correctness when some men tended to stray into dangerous by-ways.

Jenner became almost at once a national hero, and his name was nearly as well known abroad as it was in his own country. His vast correspondence led him to term himself "the vaccine clerk of the whole world". His private practice certainly suffered, and in 1802 Parliament made him a grant of £10,000. In 1806 he was voted a further £20,000. To the end he worked unceasingly for the cause of vaccination. On January 26, 1823, he passed peacefully to his rest.

Jenner never cared very much for the high honours which he received; but he did care a great deal for the practice which he had discovered and helped to foster. That is why he would have perhaps preferred an article on the movement to a eulogy of the man.

[1] Baron, J., "Life of Edward Jenner", **1**, 33 (1838).
[2] Jenner, E., *Phil. Trans.*, **78**, Pt. ii, 219–237 (1788).
[3] Baron, *op. cit.*, (1), **1**, 121.
[4] Underwood, E. A., presidential address to the Hist. Sect., Roy. Soc. Med. (1949).
[5] Baron, *op. cit.* (1), **1**, 334–39.
[6] *idem.*, *ibid.*, 392.
[7] *idem.*, *ibid.*, 359.
[8] *idem.*, *ibid.*, 155.
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[14] Spalding, J. A., "Dr. Lyman Spalding" (Boston, 1916).
[15] Lelkind, M. C., *Bull. Hist. Med.*, **7**, 671–86 (1939).
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[17] Kelly, H. A., and Burrage, W. L., "Dict. of Amer. Med. Biography" (1928).
[18] See sonnet by Basil Lanneau Gildersleeve in H. Cushing's "Life of Osler", **1**, 671.
[19] For other Waterhouse letters, see especially:
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Fitz, R., *Bull. Hist. Med.*, **11**, 239–264 (1942).
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INDUSTRIAL RESEARCH AND MANAGEMENT

A WEEK-END conference of industrial research directors and managers was held at Ashorne Hill, near Leamington Spa, during March 11–14. It was organised by the Industrial Research Committee of the Federation of British Industries, under the chairmanship of Sir Wallace Akers. The proceedings of the conference were divided into five sessions, at each of which two papers were read. Except for the first session, at which spoken contributions to the papers were made from the body of the hall, the arrangements for discussion were unusual. Immediately after the reading of the papers, the company was divided into six groups which repaired to separate rooms, where each group appointed a chairman to guide about an hour's discussion. Afterwards, the conference re-assembled and the six chairmen reported. Sometimes the groups were given specific questions to try to answer, at others their discussion was unrestricted. In either case, the method appeared to be remarkably successful in promoting full discussion of the papers in the short time available.

At the first session papers were read on the relationship between the research department and the management and other departments of an industrial organisation. Mr. F. H. Saniter described the origin and development of the United Steel Companies' Research and Development Department. It is recognized, Mr. Saniter said, that the amount of new knowledge created by an industrial research establishment is small in relation to the new knowledge continually becoming available from all sources. The Research and Development Department is therefore regarded as having three main functions: collection and dissemination of information, creation of new knowledge by research, and application of knowledge, from whatever source, to practice. The effective functioning of such a department depends on its proper integration into the general structure of the company. At the United Steel Companies this is

achieved by a series of committees ranging from a top-level committee, under the chairmanship of the managing director, downwards. These committees are considered primarily as a means of bringing people together periodically to discuss the technical needs of the firm and the ways in which they can be met by the Research and Development Department.

Dr. W. D. Scott (Monsanto Chemicals, Ltd.) gave the second paper in this session, and widened the scope of the discussion to embrace the influence of the research mind throughout the entire industrial organisation. He pointed out that there are three prime executive functions to be performed: to produce, to sell and to plan. The planning function is the intimate concern of the research department, the relationship of which to the management can be established through a number of different types of internal organisation which were illustrated by charts. Throughout, the emphasis was on a proper appraisal of the financial implications of the activities of the research department, particularly in the selection and continuation of research projects.

"Finance, Costing, Laboratory Administration and Scientific Staff, Research Programmes and their Formulation" was the comprehensive subject-heading of the second session, and both the papers and discussion which followed were of necessity selective. Mr. A. W. Montgomery (Standard Telephones and Cables, Ltd.) dealt mainly with human relationships in the research laboratory. Every establishment needs rules, however few, and since research scientists are usually knowledgeable and intelligent people, the rules of a research organisation must be reasonable and arguable. Provision must be made for staff training after engagement, and the need for adding wisdom to knowledge was emphasized. The difficult question of special treatment given to any particular member of a laboratory was touched upon, also the problem of how to treat fairly and employ correctly the older scientific worker of considerable experience who, although he has kept his knowledge up to date, has to some extent, of necessity, lost his original fire and enthusiasm. In one laboratory in the United States, such people are transferred to a group of consultants who are free of access to any individual in the company. In other places their experience is often used successfully in other parts of the company.

Another problem of great difficulty is to decide when a piece of work is complete. It is necessary to stop the work at suitable stages and consider how much can be applied, since it is through the application of research results that the upkeep of the laboratories is ultimately paid for.

Dr. I. J. Faulkner (I.C.I.), dealing with the organisation of research in the chemical industry, emphasized the necessity of not overburdening the research staff with administrative duties. It is also desirable that no individual should have to look after too many others; between four and six was considered to be the maximum. Such small teams can be easily handled, and all have a reasonably rapid approach to their chief.

A special type of organisation is required where research is carried out on a factory-scale. At the I.C.I. works at Billingham, semi-technical and pilot plant involve several hundred pay-roll employees, as well as design, construction and maintenance engineers. This is all brought under the 'research works manager', who takes off the research staff the heavy task of administering this kind of large-scale research activity. He is directly responsible, along with four other assistant research managers concerned with smaller scale research, to the research director.

The distinction between the research budget, the programme expenditure and the costing of research was also discussed by Dr. Faulkner. There are difficult decisions involved in the detailed costing of research, the allocation of costs and the definition of 'overheads', as well as in answering the all-important question, "How much should be spent on research?"

The third session, on the "Application of Research to Production", was opened with a paper from Dr. J. Park (British Oxygen Co.). Industry, and in particular the production man, the salesman and the accountant, live by what is being made now, and they inevitably acquire a vested interest in the present. The research man is concerned with what is coming next and is less interested in existing processes. The research department must win confidence at an early stage and prove that it is more than a 'frill' paid for by the profits of the other departments. It must remember that no new product is industrially significant unless somebody can find a use for it. A modest contribution to the company's present business may win more respect than a successful piece of fundamental work. Finally, the results of research must be effectively 'sold', the proper sales instrument being the concise and clearly worded research report.

Mr. L. J. Davies (B.T.H., Ltd.) classified industrial researches into five groups: (1) ancillary researches (instrumentation, measurement, etc.); (2) materials research; (3) process research; (4) design research; and (5) device research or invention. The difficulties, both technical and personal, which have to be overcome in the course of introducing new methods were illustrated by two examples taken from the electrical industry.

In the fourth session, on "Information and Intelligence, Publications and Publicity", Mr. O. W. Humphreys (General Electric Co., Ltd.) touched upon a topic which was much in evidence in the informal discussions throughout the conference, namely, the form and style of scientific reports. He said that the scientific paper enjoyable for style as well as subject-matter is all too rare; and he thinks that lectures on the English language should be included in all science and engineering courses. Any necessary reduction in the standard of the first degree would be worth it in return for some attention to the humanities.

Dr. C. G. Williams (Shell Petroleum Co.) spoke on the problem of keeping in adequate touch with the ever-rising output of technical literature. As much as can be assimilated in one to two hours a day of general technical reading was thought to be sufficient without too much interference with practical work or risk of mental indigestion. The habit of abstracting is valuable, but classification presents problems; there does not seem to be general acceptance of the Universal Decimal Classification.

The importance of presenting the results of research was emphasized by means of a formula:

$$\text{value of a research} = \text{worth-whileness of objective} \times \text{efficiency of presentation} \times \text{effectiveness of reporting.}$$

If any one of these three factors is zero, the result is zero.

Dr. L. B. Pfeil, in the first paper of the fifth session, described the relationships between the Mond Nickel

Co.'s Research and Development Department and the research associations, universities and Government departments. He made a strong plea for the expansion of Government research activities by enlargement of such establishments as the National Physical Laboratory. There is a great lack of fundamental data now badly needed by industry. Germany was, before the War, a substantial contributor to the pool of fundamental knowledge, but this source has dried up. The work of the N.P.L. and similar establishments is valuable, but much more is needed.

The universities should not generally be used as consultants by industry, but should concentrate rather on the task of providing men of sound character and personality who can think honestly and convey their thoughts with facility to others. Government grants to students and research workers are more helpful in this direction.

Mr. R. M. Winter (I.C.I. Ltd.) urged that fundamental research at the universities should not be swamped by purely tutorial duties. The training of first-class research workers is of more importance that a large output of technicians trained mainly in manipulative skills and laboratory methods. The present urgent requirements for scientific man-power should not be allowed to lower the standards of training of fundamental scientific workers. The Government's spending of £69 millions in 1947–48 on research and development, of which recurrent grants to universities and similar institutions accounts for only £3¼ millions, invites comment, Mr. Winter said.

The discussions which followed each of the five sessions were so varied and often lively as to make them difficult to summarize. There appeared, however, to be a general concern over both the quantity and quality of new recruits to the industrial research laboratories. The problem of quantity is largely a result of heavy demand coupled with interruptions to the normal flow of graduates from the universities caused by the War and by the subsequent arrangements for military service. On the side of quality, there was a strong feeling that scientific education should be on broader and less specialized lines, particularly up to the first degree.

As regards the conference as a whole, there was a general impression that it had provided an excellent opportunity for the airing and discussion, between research executives in different industries, of many common problems and interests.

W. C. F. Hessenberg

OBITUARY

Dr. G. Shearer

George Shearer died on March 13 in Addenbrookes Hospital, Cambridge, on the eve of his fifty-ninth birthday.

Shearer entered the University of Edinburgh from George Watson's College in 1907, having taken high places in both the John Welsh Classical and Mathematical Bursaries lists: for his first session he read Latin and Greek, and graduated as M.A. and B.Sc. in 1912 in mathematics and natural philosophy. He remained at Edinburgh as an assistant in natural philosophy until the outbreak of the First World War, and during this period published his first papers, on atmospheric electric potential, and Fourier and periodogram analysis, in collaboration with Dr. G. A. Carse; during this period, too, he began, under Barkla, his X-ray work, which was to be the main subject of his scientific work for the next quarter of a century. As an instructor lieutenant in the Royal Navy during 1914–19 he served afloat on navigational and gunnery duties.

On his release from war service, Shearer resumed his X-ray researches, going to Emmanuel College, Cambridge, as an 1851 Exhibition research student, and working at the Cavendish Laboratory under Rutherford. In 1922 he moved to London, as chief research assistant to Sir William Bragg, first at University College and, from 1923, at the Davy Faraday Laboratory, Royal Institution. During this period he was particularly interested in the newly discovered technique of X-ray photography of powders, and developed the application of this method to the study of the geometry and the symmetry, both of the crystalline arrangement and of the molecules themselves; one of his outstanding achievements was the X-ray chemical analysis and identification of long-chain ketones.

In 1927 Shearer was appointed to the National Physical Laboratory as a senior assistant to take charge of the work for the Committee on the Application of X-ray Methods to Industrial Problems. The group under his charge developed and used both X-ray and electron diffraction techniques, and devised various special items of equipment, such as a rotating anode tube. The problems tackled included the structure of many industrial materials (special steels, paint materials, carbon blacks, insulating materials, etc.) and the basic properties of metals. On the outbreak of the Second World War, he was transferred to the Metrology Division for work on gauges for war production. In 1941 he was seconded, and in 1948 was posted, to the Research Department (now the Armament Research Establishment) of the Ministry of Supply, in which he served until his death; in this last appointment he returned to Cambridge in charge of an out-station, and in 1947 his promotion to senior principal scientific officer enlarged his responsibilities to include the charge of other out-stations. The group of which he was the inspiration was studying the penetration of armour by projectiles, and their work contributed largely to the effectiveness of the British anti-tank artillery and aircraft cannon. Shearer's encouragement and guidance greatly facilitated the application to ballistic problems of such techniques as 'Arditron' flash photography and Kerr cell shutters.

Shearer was granted the degree of Ph.D. at Cambridge in 1921 and D.Sc. at Edinburgh in 1923 for his X-ray work. He was a fellow of the Institute of Physics and of the Physical Society, and a member and past president of the British Institute of Radiology. He also served as a member and chairman of the Committee of Visitors, and as a member of the Committee of Managers, of the Royal Institution.

On his return to Cambridge in 1941, Shearer again had rooms in his old College, Emmanuel. This was a great joy to him, although the arduous nature of his duties left him little leisure for his favourite recreations, bridge and golf, at both of which he was no mean performer. During these years he made a real contribution to the social life of the High Table and the Parlour of the College, and his wide interests and broad sympathies made him a gracious host to many official visitors. An able physicist, a wise administrator, and a modest man, he will be missed by many friends.

NEWS and VIEWS

Biochemistry at Cambridge : Prof. A. C. Chibnall, F.R.S.

PROF. A. C. CHIBNALL, whose impending retirement from the Sir William Dunn chair of biochemistry has recently been announced, has held his present position since 1943. During his period of tenure, the Department of Biochemistry at Cambridge has fully maintained its former distinguished record. Under his guidance there have not only been developments in the major themes of research on the dynamic aspects of biochemistry for which the Department became famous under the late Sir Frederick Gowland Hopkins ; in addition, much more attention has been paid than in the past to the biochemistry of proteins, a subject in which Prof. Chibnall himself is especially interested. As might be expected also, plant biochemistry at Cambridge has received much encouragement at his hands. A notable development which has occurred under Prof. Chibnall is the expansion of work on microbiological chemistry ; this has led to the recognition of microbiological chemistry in the Natural Sciences Tripos, and to the establishment of a major division of the Department, headed first by the late Dr. Marjory Stephenson and now by Dr. E. F. Gale, with responsibility for research and the training of students in this branch of biochemistry. Although Prof. Chibnall's tenure of the chair at Cambridge has been short, he has thus very definitely left his mark on the Department. The regret of his scientific colleagues at his early retirement will be tempered by their knowledge of his intention to remain in Cambridge and to devote to scientific research the time liberated by relief from duties of administration and teaching. Doubtless also his wide knowledge and experience will continue to be available to such bodies as the Agricultural Research Council, to which he has given so much help in the past.

Prof. F. G. Young, F.R.S.

PROF. CHIBNALL will be succeeded in October next by Prof. F. G. Young, who will thus be leaving University College, London, after having held the chair of biochemistry there since 1945. During the War the Biochemical Department of University College was evacuated from London, and one of his main tasks on appointment in 1945 was to superintend its return and to adapt it to post-war needs. This he has done with conspicuous success. Not only have the laboratories been more or less completely re-equipped, but also the teaching provided has been brought up to date by the introduction of a new course leading to the degree of M.Sc. in biochemistry. Prof. Young's research interests are in the sphere of carbohydrate metabolism, and brief mention of his activities to 1945 was made in a previous note (*Nature*, **156**, 471 ; 1945). His outstanding achievement has been the production of permanent experimental diabetes in animals by injection of anterior pituitary extracts, and he has subjected the whole question of pituitary control of the insulin-secreting function of the pancreas to close and skilful experimental analysis. The value of his work was recognized recently by his election to the fellowship of the Royal Society. Under Prof. Young's vigorous leadership, the Biochemical Department at University College has become once again a flourishing centre for teaching and research, and his pending departure will be viewed by his colleagues there with great regret. He goes to Cambridge with an established reputation, and he carries with him the good wishes of his numerous friends at home and abroad.

Specialization at Universities

THE numbers of university undergraduates, in the technical faculties especially, are to-day greater than ever before, and yet apprehension as to the true worth of university training was never so widespread as at the present time ; for modern scholarship has made such strides, particularly in the field of science, that no one subject can be adequately studied without undue specialization and consequent neglect of other points of view. This danger of specialization was the theme of Mr. Oliver Stanley's recent address, on being installed as chancellor of the University of Liverpool, when he said that too many men and women to-day leave a university complete masters of a subject but still incomplete individuals, unable to act as evangelists of that broader culture, that more general philosophy which should be the university's gift to the people. These words were echoed by Mr. Winston Churchill when, on the same occasion, he received the honorary degree of doctor of laws. Indeed, Mr. Churchill went further and, to a certain extent, accused science of being the usurper of culture. Few men of science would seriously dispute that ". . . We need a lot of engineers but we do not need a world of modern engineers. . . . Science must be the servant and never the master of mankind" ; but Mr. Churchill's further statement, "I would venture to say that no amount of technical knowledge can replace the value of the humanities or the study of history", is one that is open to challenge. The study of science need not preclude a deep appreciation of the humanities ; rather it is the high degree of specialization within the syllabus which is the excuse for not teaching a general appreciation of scientific principles. Many physics graduates have very little knowledge of the processes of life, just as, say, the majority of botanists are probably unaware of even the elementary facts of the quantum theory or relativity ; and both know very little of either the history or philosophy of science. It is on this last point that, no doubt, science is lacking as a medium for educating (in the widest sense of the word) the student. If the teaching of science itself were less specialized and more cultural, then perhaps a scientific course would be more readily recognized as being as good an education as, say, a classical one, for training the undergraduate to become a leader of thought and action.

New Coalfield in Britain

IT has been announced by the Lord President of the Council that a new coalfield has been discovered in south Staffordshire during the course of deep boring by the Geological Survey of Great Britain. At a minimum estimate this coalfield contains 400 million tons of workable coal, and the amount may possibly be several times as great as this estimate. The seams lie at a depth of about 3,000 ft. ; thirty feet of coal has so far been discovered, including seams of 8, 6 and 5 ft. Boring is still going on, and it is likely that further seams will be found. The boring is at Whittington Heath near Lichfield. The new coalfield is situated between the Cannock Chase coalfield and the Warwickshire coalfield, in both

of which the productive coal measures crop out at surface. Additional borings will be necessary to prove the full resources and detailed structure of the new field. Funds to undertake a programme of deep boring were granted to the Geological Survey in 1947 for the purpose of investigating the deep-seated geological structure of Great Britain. It is unlikely that all the bores will give direct economic results of such importance as the Lichfield bore; but the information gained from the bores will enable geologists to interpret the structure of the rocks at great depths beneath considerable areas of the country. Knowledge of this kind is of great general importance in the search for underground mineral and water resources. Mr. T. Eastwood, assistant director of the Geological Survey in charge of mineral deposits of England and Wales, is responsible for the work. Boring has been carried out by the Craelius Company under contract.

Universities of the British Commonwealth: Congress in Canada

A MEETING of representative vice-chancellors and principals of the various countries of the British Commonwealth is to be held in Canada next month. At the Congress of Universities of the Commonwealth held at Oxford in July 1948, it was decided that there should be smaller meetings of executive heads of universities annually in some part of the Commonwealth in order to discuss matters of common policy, including the implementation of the decisions of the Congress on inter-university relationships (interchange of staff, students, etc.). This, the first of such conferences, will take the form of a meeting of the Executive Council of the Association of Universities of the British Commonwealth, and will be held at Halifax, Nova Scotia, during June 10–11, followed by the annual meeting of the National Conference of Canadian Universities during June 12–15, at which the overseas representatives will be guests. The conference will conclude with a meeting in Montreal on June 17. The president of McGill University, Dr. F. Cyril James, will preside, and the local arrangements are in the hands of Mr. T. H. Matthews, registrar of McGill University and secretary–treasurer of the National Conference of Canadian Universities.

The following is a list of delegates who will be attending the Conference. *Great Britain and Ireland:* Sir David Lindsay Keir (vice-chancellor, Queen's University, Belfast), Very Rev. Dr. J. Lowe (vice-chancellor, University of Oxford) and Prof. Lillian Penson (vice-chancellor, University of London). *Canada* (representatives on Executive Council): Dr. F. Cyril James (principal, McGill University, Montreal), Monseigneur F. Vandry (rector, Laval University, Quebec) and Dr. R. C. Wallace (principal, Queen's University, Kingston, Ontario). *Australia:* Prof. D. B. Copland (vice-chancellor, Australian National University) and Prof. W. Prest (dean of the Faculty of Commerce, University of Melbourne). *South Africa:* Dr. E. G. Malherbe (principal, University of Natal) and Dr. H. R. Raikes (vice-chancellor, University of the Witwatersrand). *New Zealand:* Dr. R. S. Aitken (vice-chancellor, University of Otago). *India and Ceylon:* Dr. P. N. Banerjee (vice-chancellor, University of Calcutta) and Sir A. Lakshmanaswami Mudaliar (vice-chancellor, University of Madras). *Pakistan*: Dr. S. M. Hossain (vice-chancellor, University of Dacca). *Other parts of the Commonwealth:* Colonel Lindsay Ride (vice-chancellor, University of Hong Kong). *Officers of the Association:* Sir William Hamilton Fyfe (honorary treasurer, formerly vice-chancellor, Queen's University, Kingston, Ontario, and University of Aberdeen), Mr. J. F. Foster (secretary of the Association and of the Committee of Vice-Chancellors and Principals of Great Britain and Ireland, formerly registrar of the University of Melbourne, Australia) and Miss Margaret Palstra.

Petroleum Oils and the Fruit-grower

A LECTURE given by Dr. T. Swarbrick to the Royal Society of Arts, on January 26, described, *inter alia*, some interesting features of the use of petroleum oils for controlling the pests of fruit trees. One difficulty about their use was an occasional phytotoxic effect. The chemists of the Royal Dutch Shell Group have now traced the cause of this damage, and found methods for its removal. Earlier work by Chapman and Pearce indicated the necessary conditions of boiling point and viscosity which conferred insecticidal action. Petroleum can also be used as a total or selective weed-killer, and new methods of application are designed to reduce the amount of water required in spraying. Atomized oil sprays may need no more than 2¼ gallons of spray fluid per acre for crops of small fruit.

A Virus-inhibiting Protein

A GLYCOPROTEIN which can inhibit the activity of tobacco mosaic, tomato bushy stunt and other plant viruses has been isolated by B. Kassanis and A. Kleczkowski (*J. Gen. Microbiol.*, **2**, No. 2; May 1948). It was obtained from the sap of *Phytolacca esculenta* by differential precipitation with ethanol followed by adsorption on 'Celite' and elution with 10 per cent sodium chloride. The protein is unaffected by pepsin and trypsin unless it is denatured, and it is not effective against bacteriophage.

Royal Institute of Chemistry

AT the annual general meeting of the Royal Institute of Chemistry held on April 28, it was announced that H.M. the King had been graciously pleased to grant to the Institute on April 14 a new royal charter in substitution for the original charter of 1885 and the supplemental charter of 1944. In the new charter it is made clear that the Institute is concerned with the whole profession of chemistry and not merely with that of "analytical and consulting chemistry", as stated in the original charter. The powers of the Institute have been modified in certain respects so as to facilitate the progressive development of its activities and services in the interests of the profession and of the public. The title of the Institute has been changed from "The Royal Institute of Chemistry of Great Britain and Ireland" to "The Royal Institute of Chemistry". It has been resolved to amend the by-laws so as to provide that citizens of Eire, as well as British subjects, shall be eligible for admission as members or for registration as students of the Institute. Resolutions were passed at the meeting authorizing the Council to establish, as a benevolent fund, a Residential Clubs Fund.

The following officers for 1949 were elected: *President*, Prof. J. W. Cook; *Vice-Presidents*, Prof. F. Challenger, Mr. H. Krall, Prof. R. P. Linstead, Mr. G. Roche Lynch, Mr. J. A. Oriel, Mr. E. T. Osborne; *Honorary Treasurer*, Dr. D. W. Kent-Jones.

Physical Society: Summer Meeting at Cambridge

THE summer meeting of the Physical Society will be held this year in Cambridge during July 14–16. The main subject will be "The Ionosphere and the Propagation of Radio Waves". Among the speakers will be Prof. S. Chapman (Queen's College, Oxford), Mr. J. A. Ratcliffe (Cavendish Laboratory, Cambridge), Dr. A. C. B. Lovell (University of Manchester), Mr. G. Millington and Mr. S. B. Smith (Marconi Wireless Telegraph Co.), and representatives from the Radio Research Board and the British Broadcasting Corporation; visitors from France, Norway and Sweden are also expected. Attendance at the Conference is free to all members of the Physical Society; a fee of 10*s.* a day will be charged to non-members. Further details may be obtained from the offices of the Physical Society at 1 Lowther Gardens, Prince Consort Road, London, S.W.7. Non-members of the Society should apply by May 31.

Geological Society of London: Officers for 1949

AT the annual general meeting on April 27 of the Geological Society of London, the following officers and members of the Council were elected for the coming year: *President*, Prof. C. E. Tilley; *Vice-Presidents*, Prof. J. A. Douglas, Prof. L. Hawkes, Dr. G. M. Lees and Prof. H. H. Read; *Secretaries*, Dr. D. Williams and Prof. S. E. Hollingworth; *Foreign Secretary*, Prof. O. T. Jones; *Treasurer*, Mr. P. Evans. *Other members of Council:* Dr. A. J. Bull, Mr. A. J. Butler, Dr. F. Dixey, Dr. K. C. Dunham, Dr. W. L. F. Nuttall, Dr. K. P. Oakley, Dr. J. Phemister, Prof. W. J. Pugh, Prof. F. W. Shotton, Dr. W. Campbell Smith, Dr. C. J. Stubblefield, Prof. D. M. S. Watson, Dr. E. I. White and Prof. Alan Wood.

University of Sheffield

THE Council of the University of Sheffield has made the following appointments: B. R. Dudley, at present research assistant to the Professor of Engineering, to be lecturer in the Department of Mechanical Engineering; and Dr. Hsun Lee, at present independent research worker for the British Iron and Steel Research Association, to be research fellow in the Department of Metallurgy.

The following resignations were announced: Dr. J. A. Pope, senior lecturer in mechanical engineering, on his appointment to the chair of engineering in the University of Nottingham; Dr. J. H. Bowie, lecturer in bacteriology, on his appointment to the post of bacteriologist at the Royal Infirmary, Edinburgh; Dr. A. E. Bender, assistant lecturer in biochemistry, on his appointment as leader of a nutrition research team at the Crookes Laboratories.

The Night Sky in June

FULL moon occurs on June 10d. 21h. 45m., U.T., and new moon on June 26d. 10h. 02m. The following conjunctions with the moon take place: June 3d. 04h., Saturn 3° S.; June 13d. 22h., Jupiter 5° N.; June 24d. 15h., Mars 4° S.; June 24d. 18h., Mercury 8° S.; June 27d. 21h., Venus 4° S.; June 30d. 13h., Saturn 2° S. Mercury is in inferior conjunction on June 3 and is stationary on June 15. The planet reaches greatest western elongation on June 28, and at the end of the month rises an hour before the sun. Venus is an evening star, setting about an hour after the sun during June, and can be seen for a short time in the western sky. Mars is a morning star, rising at 3h. 10m., 2h. 40m., and 2h. 12m., at the beginning, middle and end of the month, respectively, and is visible for a short time in the eastern sky. Jupiter is visible in the early morning hours, rising at 23h. 30m., 22h. 33m., and 21h. 30m. on June 1, 15 and 30, respectively. Saturn is visible during the earlier portion of the night, setting at 0h. 40m., 23h. 45m. and 22h. 50m. at the beginning, middle and end of the month, respectively. No occultations of stars occur during June. Summer solstice commences on June 21d. 18h.

Announcements

THE Stephen Paget Memorial Lecture of the Research Defence Society will be delivered by Prof. C. Lovatt Evans, on the occasion of the annual general meeting of the Society on June 9; he will speak on "Physiological Research and the Vivisection Act".

COMMDR. R. G. A. JACKSON, of the Treasury, will examine with, and report to, the secretary of the Department of Scientific and Industrial Research, Sir Ben Lockspeiser, the administrative arrangements by which the Government is promoting the application of scientific knowledge to problems of productivity and economic development. Commdr. Jackson was director-general of the Middle East Supply Centre during the War, and later became an assistant secretary-general of the United Nations.

MR. E. C. CHERRY has been appointed to the Henry Mark Pease readership in telecommunications tenable at the Imperial College of Science and Technology, London, as from October 1.

THE seventh International Botanical Congress will be held in Stockholm during July 12–20, 1950. Excursions in connexion with the Congress will start on June 27, and others to follow it will end on August 9. A preliminary programme and excursions can be obtained from the Secretary-General, Dr. Ewert Åberg, Uppsala 7.

THE fourth Annual Electronics Exhibition of the North-Western Branch of the Institution of Electronics will be held at the College of Technology, Manchester, during July 19, 2.30 p.m.–9 p.m., and July 20–21, 10 a.m.–9 p.m. There will also be an exhibition of scientific films arranged by the Manchester Scientific Film Society. Admission to the Exhibition will be by ticket only, obtainable from Dr. J. A. Darbyshire, 'Melandra', 1 Kershaw Road, Failsworth, Manchester.

THE French Physical Society is holding an Exhibition of scientific instruments and apparatus in the Salons de l'Université, Sorbonne, at 47 Rue des Écoles, Paris 5, during June 4–9. Further information with regard to the Exhibition may be obtained from M. Marandon, 61 Avenue de l'Observatoire, Paris 14. The Society is also holding a meeting on June 4 at which the Holweck Prize will be presented to Prof. L. F. Bates, of the University of Nottingham, who will give a lecture on "Thermal Effects due to Magnetization Processes in Weak Fields".

A SUMMER school in the theory and application of the electron microscope will be held in the Cavendish Laboratory, Cambridge, during August 16–17. The course is intended for those who will be using electron microscopes in the near future, and the techniques of preparing specimens and of operating various microscopes will be practised. Further information can be obtained from Mr. G. F. Hickson, Secretary of the Board of Extra-Mural Studies, Stuart House, Cambridge.

LETTERS TO THE EDITORS

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Further Studies in Synthetic Polypeptides

OUR investigations of synthetic polypeptides, which have been proceeding concurrently with, although independently of, those described by Astbury and Dalgliesh[1], were also inspired by the announcement of the discovery of a benzene-soluble co-polymer prepared from anhydro-N-carboxy-DL-β-phenylalanine and L-leucine by Woodward and Schramm[2] using the Leuchs polymerization[3].

We would confirm the results of Astbury and Dalgliesh[1] that the majority of the synthetic polypeptides so far prepared give X-ray powder photographs resembling those given by β-proteins. We found, however, that the copolymers (1 : 1 mol.) of DL-β-phenylalanine and L-leucine, as prepared originally by Woodward and Schramm[2], and of DL-β-phenylalanine and α-amino-*iso*butyric acid, give X-ray powder photographs the two principal diffraction rings of which correspond to inter-planar spacings of 5·2 and 11·7 A. Films cast from solutions of these co-polymers in benzene, when examined with the X-ray beam parallel to the film surface, give an oriented diffraction pattern (Fig. 1) which is strikingly similar to those given by α-proteins. Some of these preparations of film are extensible, but stretching, even up to 100 per cent, does not appear to affect the X-ray photograph.

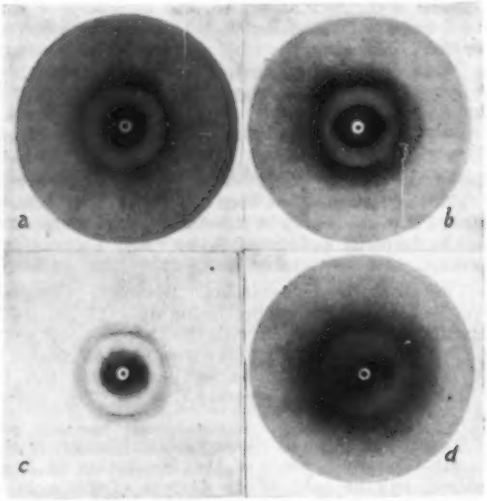

Fig. 1. X-Ray diffraction photographs of synthetic polypeptides: (*a*) film, stretched 50 per cent, with X-ray beam perpendicular; (*b*) film, stretched 50 per cent, with X-ray beam parallel to surface; (*c*) fibres, taut; (*d*) natural α-keratin, human hair, for comparison with (*c*)

In addition, we have now spun fibres of the DL-β-phenylalanine-L-leucine co-polymer. The X-ray photograph of these shows one marked difference from the photograph of the films. While the strongest meridional spacing remains at 5·2 A., the 11·7 A. reflexion becomes oriented on the equator in place of the 10·4 A. reflexion, which disappears altogether from the equator (see Fig. 2).

The similarity of the photograph of the synthetic fibres to that of natural keratin leads to some interesting suggestions regarding their structure. In the first place, we cannot be absolutely certain of the identity of the two photographs; but the spacings are so nearly the same and the general character of the photographs are so similar that we feel them to be produced by essentially the same type of molecular configuration. Up to the present, we have not been able to extend these synthetic fibres into the β-configuration, so complete analogy with natural keratin has not yet been established. The repeat period of the synthetic fibres is 10·4 A., whereas in silk with a fully extended peptide chain it is ~ 7 A., which is in agreement with the length calculated from known bond-lengths and -angles. This fact leads us to suggest that in protein fibres of the α-keratin class, both natural and synthetic, the polypeptide chains do not lie parallel to the length of the fibre, but transverse to it. If this were so, the structure would be held together along the fibre by hydrogen bond systems and by the interlocking of the side-chains (Fig. 3). Experiments are in progress to test the truth of these hypotheses.

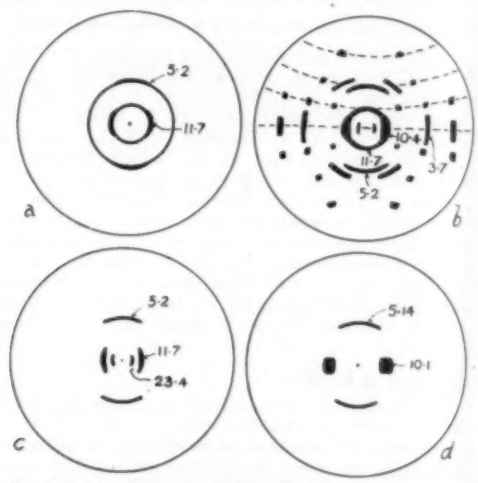

Fig. 2. Inter-planar spacings measured from corresponding photographs in Fig. 1

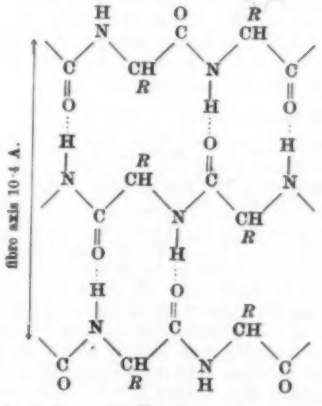

Fig. 3

The molecular weights of the films have been estimated by end-group methods; the nitrogen liberated in a Van Slyke amino-nitrogen apparatus, and the absorption of free carboxyl groups by methylene blue, both point to a figure in the neighbourhood of 15,000, very much lower than the "several million" quoted by Woodward and Schramm[2].

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Organisms Producing Antibiotics in Tropical Soils

Except for some experiments by Meredith[1] with Jamaican soils, little information is available on the presence in tropical habitats of antibiotic-producing micro-organisms. Using a technique similar to that of Stokes and Woodward[2], we are finding increasing evidence that tropical soils harbour such organisms, sometimes types which have not yet been identified.

Actinomycetes, as was to be expected, are widely represented, but in our opinion are of much less interest than certain spore-forming and non-spore-forming aerobic bacteria which occur there. These produce a surprisingly selective inhibition of the growth of other organisms.

A short rod, for example, discovered by one of us (I. T.) in Trinidad air, and presumably carried there with soil particles, produces a water-soluble thermostable and apparently non-toxic antibiotic which arrests the growth of all the fungi and yeast tested —so far more than forty species. Its action is often a positive one in concentrations of less than one part in a million. Against Gram-positive and Gram-negative bacteria this antibiotic is inactive.

Another organism which has been isolated is active against fungi, bacteria (*Bact. coli commune*) and aerobic bacilli (*Bact. mycoides*), while others are effective against fungi and bacteria.

In one of the investigations now proceeding the presence of peptone in the culture medium is essential for the production of an antibiotic. In another series glucose and peptone are required. It has not yet been ascertained whether peptone can be replaced by amino-acids, and, if so, by which.

It is proposed to extend the study now in progress to cover tropical habitats other than soils, and to alter the technique employed so that organisms may be collected—if they should exist—which may require materials other than peptone and glucose for antibiotic production.

In his review of bacteria in soil, Conn[3] refers to the isolation of antibiotic organisms from soils. He admits the interest of such work, which he classifies as both important and practical, though not agriculturally significant. We suggest that the existence of organisms producing antibiotics may play a part in maintaining a balanced microflora in soil. In this we are in full agreement with the views of Brian[4] expressed recently.

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Concentration of the Toxic Substance from 'Agenized' Flour

It was shown by Sir Edward Mellanby[1] that flour, treated with nitrogen trichloride, as it is in the commercial 'Agene' process, is capable of causing fits in dogs, when fed in fairly large quantities. Further investigations have proved that the toxicity is associated with the gluten of the flour, and that the toxicity survives digestion with pepsin and with trypsin[2]. The recent publication in abstract by Reiner *et al.*[3] of particulars of the fractionation and concentration of the toxic substance (mainly prepared by the action of nitrogen trichloride on zein), and the publication by Moran *et al.*[4] of a method of concentration of the toxic substance from zein, make it desirable for us to give our own results.

The process employed involves pancreatic digestion of the gluten followed by passage through resin columns and precipitation with mercury acetate. The gluten was not previously separated from the flour. 70 lb. of flour 'Agenized' at the rate of 125 gm./sack was mixed with 14 gallons distilled water, 500 gm. pancreatin and 500 ml. chloroform. The mixture was adjusted to *p*H 8 with sodium hydroxide and disodium hydrogen phosphate and digested at 37° for 24 hr. The sludge was centrifuged. The recovered gluten was re-suspended in about 3 gallons of water, brought to *p*H 8, and a further 500 gm. pancreatin added. This mixture was digested for a further 48 hr. at 37° and centrifuged. The supernatant from this digestion was usually more toxic than that from the first. It was treated with 2 vol. of absolute alcohol, and the precipitate formed was centrifuged off. The alcoholic liquor was passed through a column containing 2½ lb. of activated 'Zeocarb 215'. When the adsorbed material was eluted with dilute sodium hydroxide, the toxicity was found in the neutral 'fraction' associated with some residual quantities of the acidic and basic amino-acids.

The concentrate so obtained was treated with acid-activated alumina, which removed the acidic amino-acids. After extraction with butanol, a product was obtained which showed four spots on two-dimensional paper chromatography, three of which could be identified as glycine, serine, alanine, and the fourth as glutamine running with an almost indistinguishable unknown substance.

In order to confirm the possibility that this unknown spot was associated with the toxic substance, a quantity of the concentrate was placed in a line along the edge of a large sheet of filter paper and developed with phenol in a direction at right angles to the line. The unknown spot was then located in a long streak parallel to the original position. This part of the chromatogram was eluted with water, and

the presence of the toxic substance in it was confirmed by feeding tests.

Later it was found that the unknown spot, which is shown up by the ninhydrin reaction, is accompanied by another, which does not give the ninhydrin reaction, but is detectable in ultra-violet light. As an alternative to the chromatographic separation, which is extremely laborious on a large scale, it was found that the toxic substance could be precipitated by mercuric acetate, added in 25 per cent alcohol solution acidified with acetic acid. The precipitated mercury salt was suspended in water, and hydrogen sulphide was passed to precipitate the mercury. The solution was evaporated to dryness. About 20 mgm. nitrogen of the substance obtained was required to give the toxic symptoms in a ferret.

On chromatographic examination it was found that the spot which we believed to be that of the toxic substance was not now accompanied by the fluorescent material. The product has, however, varied somewhat in different preparations, probably owing to an uneven efficiency of the separations, which are not easily controlled. In some cases the two-dimensional chromatogram shows only the one spot; in others, it has been accompanied by others representing histidine, glutamine, asparagine, valine and leucine. Traces of cysteine have also been detected in some preparations. It was later found that in some of the toxic preparations all the chromatographic spots could be accounted for, so that it is likely that the toxic substance, although closely associated with glutamine, does not itself give a ninhydrin colour. This is confirmed by the fact that a similar product from a non-toxic flour also gives the same spots. A further purification on the same scale is being attempted.

We are indebted to Prof. E. C. Dodds for his interest; to Messrs. Wallace and Tiernan for their financial support and permission to publish, and also for assistance with the large-scale digestions, which were carried out in their works; to Dr. D. W. Kent-Jones, for the preparation of the 'Agenized' flours and for constant interest and advice; and to Dr. L. Reiner, of Messrs. Wallace and Tiernan, Newark, New Jersey, for helpful discussions with the senior author during March 1948, in which the methods employed by him at that time were disclosed, and Mrs. S. Simpson for assistance with the toxicity tests.

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Influence of Desoxycorticosterone on Glycogen Formation and Glucose Uptake of Isolated Muscle

Isolated diaphragm muscle of rats produces glycogen from glucose especially in the presence of insulin in oxygenated Ringer's solution. This is inhibited by the addition of small doses of desoxycorticosterone[1]. If the muscle works in response to rhythmic stimulation for two hours, glycogen is newly formed and is broken down again. This is also inhibited by desoxycorticosterone[2]. The action of other steroid hormones was also studied. They had either equal effect (progesterone, testosterone) or none (corticosterone, etc.)[2].

We have now studied whether desoxycorticosterone also inhibits the glucose uptake[4] of the surviving isolated muscle.

The diaphragm of normal rats was cut into three pieces immediately after death. One served for the estimation of the starting value of glycogen content. The two other parts were each put in a vessel with 2 c.c. of Ringer's solution with 200 mgm. glucose and 1 unit of insulin per 100 c.c. The vessels were shaken in an oxygen atmosphere at 37° C. for 90 min. To one vessel 0·1 mgm. desoxycorticosterone was added (5 mgm. per 100 c.c.).

Thirty rat diaphragms were tested, and the results are given in mean values with the median error $(\sqrt{\Delta^2/n(n-1)})$. Besides the glycogen formation, the glucose content of the solution was also measured at the beginning and end of the experiment. The methods used were the same as before[1-3].

	Mean values		
	Glycogen production (30 experiments)	Glucose uptake	NH_3 produced (13 experiments)
Per 100 gm. muscle in 90 min.	272±37 mgm.	457±24 mgm.	8·0±0·6 mgm.
Same, with *addition of* 4·4, 5 or 7 mgm. per 100 c.c. desoxy-corticosterone	56± 5 mgm.	200±20 mgm.	9·0±1·3 mgm.

The surviving muscle about doubled its glycogen content in this period, as in former experiments[1]. It took up glucose from the solution, but considerably more glucose disappeared than corresponds to the newly formed glycogen[4]. This occurred in each single experiment and is shown in the mean value, where 272 mgm. glycogen production compares with 457 mgm. glucose consumption.

Desoxycorticosterone totally inhibited the glycogen production; but it decreased the glucose uptake only to about one half. This value seems to correspond to the difference between the glycogen produced and the total glucose consumption of the muscle without addition of desoxycorticosterone. It might be, therefore, that the remaining glucose is metabolized, and that desoxycorticosterone inhibited only glycogen formation. However, it may also be that primarily the permeability for glucose is decreased, and this accounts for the decrease in glucose uptake and in glycogen formation. We intend to study this problem further.

In thirteen experiments, the ammonia production of the surviving muscle was estimated in the solution, using Conway's method[5]. It seemed possible that after the inhibition of glycogen formation, the muscle increased protein breakdown through desamination. No increase of ammonia production was found.

We thank Ciba A.G. for providing us with desoxycorticosterone.

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Distribution of Ribonucleic Acid in the Cytoplasm of Growing Cells Studied with Phosphorus-32

THE occurrence of ribonucleic acid in the submicroscopic particles obtained by centrifugation of extracts of organs (granules, microsomes, liponucleoprotein complexes) has suggested the hypothesis that these particulate structures might have a role in the synthesis of proteins, and that they may even be identifiable with the viruses and plasmagenes. This idea implies not only that these particles actually exist in the cytoplasm but also that ribonucleic acid is really part of them. It is possible, of course, that they are artefacts which appear upon homogenization of the cells. Indeed, Kleczkowski[1] and Lauffer[2] have shown that a nucleoprotein, the tobacco mosaic virus, is able to form complexes with different proteins under the influence of small changes in the medium, and it is also known that the distribution of ribonucleic acid among the different fractions separable by ultracentrifugation of a tissue extract depends on the salt concentration of the extraction medium[3]. Brachet and Jeener[4] observed in 1943 that high-speed centrifugation of a piece of frog liver displaced a large part of the ribonucleic acid towards the centrifugal ends of the cells; but this phenomenon was not seen when mammalian liver was used. Furthermore, the conditions under which centrifugation is carried out are quite abnormal (using concentrated sugar solutions of the same density as the tissue).

It appeared that the use of labelled atoms might give some idea as to the distribution of the constituents of the granules in the cytoplasm, when the latter is maintained under strictly physiological conditions. Labelled phosphorus was injected into gravid mice, hen's eggs or into pigeons, the crop glands of which were actively proliferating after injections of prolactin. The mouse embryos, chick embryos or crop glands were removed two hours after the injection of the labelled phosphorus and mechanically homogenized with phosphate buffer at *p*H 7·3 containing 30 per cent sucrose. The extract thus obtained was cleared of nuclei and cellular debris by a short centrifugation at low speed and then fractionated by further centrifugation into three parts: (*a*) the 'pellet' of granules obtained after centrifuging for 14 min. at 13×10^3 *g*, (*b*) the pellet resulting from 60 min. centrifugation at 60×10^3 *g*, and (*c*) the supernatant liquid resulting from (*b*). The specific radioactivity of the phosphorus of the ribonucleic acid was determined by the Schmidt–Thannhauser method as modified by Spiegelman and Kamen[5]. In order to get rid of the inorganic labelled phosphate ions an excess of normal phosphate ions was added and the whole eliminated as ammonium magnesium phosphate. A control experiment showed that the ratio between optical density at 255 mμ and the phosphate hydrolysed by 1 *N* hydrochloric acid in three hours at 100° C. was the same in the case of the solution of mononucleotides studied and of a similar solution prepared from purified yeast ribonucleic acid.

As the accompanying table demonstrates, the specific radioactivity of the phosphorus of the ribonucleic acid of each of the three fractions is quite different (there is a probable error of ± 4 per cent). However, it is still impossible to tell to what degree the labelled phosphorus found in the nucleic acid of the different fractions is a measure of the renewal of nucleic acid molecules or of an increase in their number during the growth of the cells.

Specific radioactivity of phosphorus in ribonucleic acid (in arbitrary units per 100 γ P)

	Supernatant of 60×10^3 *g*	Pellet of 60×10^3 *g*	Pellet of 13×10^3 *g*
Mouse embryo	100	16	75
	124	38	82
	605	223	498
	1365	440	1000
Crop gland of pigeon	316	272	475
	5960	143	5170
Chick embryo	155	97	101
	263	45	37

But an important conclusion can be drawn from the facts just stated. It is that nucleic acid is seen to be at least partially associated with particles, and that these really exist in the cytoplasm of the living cell. It would be difficult to explain otherwise how the radioactivity of the phosphorus of the nucleic acid differs from one fraction to another, for such would certainly not be the case if nucleic acid existed in a free state in the cell, associating with other substances to form granules only as a result of the extraction procedure. The results show further that at least three different fractions of ribonucleic acid exist, as well as at least two kinds of granules which contain it.

It may also be pointed out that it seems quite certain that the differences in specific radioactivity which have been observed are definitely present in the living organism and not merely appearing in the extracts during the various procedures, for the turnover of nucleic acid phosphorus in the extracts is three hundred times too small to account for the differences seen.

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The Weiss–Heisenberg Theory of Ferromagnetism and a New Rule Concerning Magnetostriction and Magneto-Resistance

IT is considered an open question[1] whether the Weiss–Heisenberg model or the band model, which is based on the conception of the itinerant electron, is the more appropriate one for the description of ferromagnetic phenomena. On the whole, the original Weiss treatment translated into modern language is surprisingly powerful when compared with numerous later attempts at an improvement.

It occurred to me that arguments for or against the Heisenberg model might be derived from a consideration of the properties of ranges of alloys in which the mean number of Bohr magnetons passes through an integral value. The Heisenberg model leads one to expect that the exact filling up of all lattice-points with an equal number of electrons will make itself felt in some properties of the ferromagnetic alloy. From the point of view of the band theory, however, a singling out of such an alloy from

its neighbouring alloys is pointless, there being no reason why this particular alloy should be different from other alloys. We therefore have here a suitable criterion at hand for deciding between both theories.

In order to have a reasonable chance of finding such a singularity at integral values of the mean number of Bohr magnetons, it is, of course, preferable that the alloy components shall be chemically as much alike as possible, and that the lattice structure should be a simple close-packed arrangement.

The above conditions are fully met by a nickel–cobalt alloy containing 33 per cent of cobalt[2], by a nickel–iron alloy containing 18 per cent of iron[2] and approximately by an alloy of composition Ni_2Mn in the ordered arrangement[3]. In the first two of these alloys, the mean number of Bohr magnetons is equal to one, in the third it comes close to one (about 0·95).

From a survey of the available literature[4,5,6], it became at once clear that our criterion leads to alloys having zero magnetostriction, a most remarkable result indeed. For the nickel–cobalt series the agreement between the two critical concentrations at first seemed rather poor. As pointed out by Kühlewein[7], however, the existing very old data on the magnetostrictive properties of nickel–cobalt alloys are not to be trusted, probably due to lack of purity of the cobalt. Control measurements which have been carried out on high purity nickel–cobalt alloys by J. J. Went of this Laboratory have confirmed our expectation[8].

An extended search in the literature further brought to light that at the concentration where $\lambda = 0$ (and accordingly $n_B = 1$), the change in the electrical resistance due to the application of a magnetic field passes through a maximum.

For the nickel–iron series only measurements carried out at room temperature are available[9], and even these are scanty. On nickel–cobalt, however, extensive measurements have been carried out by Shirakawa[10] at various temperatures down to −195° C. At this temperature the effect is very pronounced, and the maximum change observed (16 per cent) is found to occur in alloys with between 30 and 40 per cent of cobalt. At room temperature the maximum is much weaker (only 5 per cent), and at 300° C. it is no longer observable.

It is tempting to speculate a little further on the anisotropy of the magnetostriction as observed by Lichtenberger[11] on single crystals of nickel–iron. Apart from minor discrepancies, Lichtenberger's results can be adequately summed up in the following statement: apparently the absolute value of λ always has a minimum value in the direction where the internal energy also has a minimum value.

An extension of the results obtained above to other cases is difficult and of doubtful value. Alloys of ferromagnetic components, having, in the mean, two Bohr magnetons instead of one, no longer have a close-packed structure, but are cubic body-centred. All the same, on the whole the magnetostriction seems to be small also in those alloys, so there is no contradiction. For alloys of a ferromagnetic component with an unmagnetic component, one does not expect a simple rule like the one stated above to hold, because of the very different character of the components. Indeed, it is found that their behaviour is quite different from that of alloys having only ferromagnetic components.

Alloys like Fe–Pt, where n_B is also found to pass unit value, are subject to phase transformations and therefore difficult to investigate.

It is felt that the observed near coincidences in the nickel–cobalt, nickel–iron and nickel–manganese binary systems, as well as the above provisional rule on the anisotropy of the magnetostriction, may perhaps serve as a starting point for a theory concerning these phenomena.

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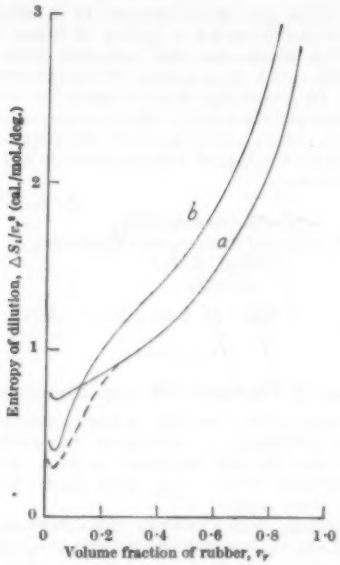
Entropy of Dilution of a Polymer-Solvent System

In examining the properties of polymer-solvent systems, considerable attention has been given in the last decade to the evaluation of the configurational contribution to the partition function. It has been shown[1] that the greater part of the divergence of these systems from ideal behaviour is due to the vast differences in the sizes and shapes of the molecules of the polymer and of the solvent. This configurational contribution exercises a dominating influence on the thermodynamical properties of polymer-solvent systems. The rubber–benzene system has been most thoroughly investigated experimentally. The theory reproduces the observed behaviour of the free energy, and also the general trend of the entropy at ordinary concentrations (see ref. 1, sections 4.6, 4.9, 4.10 and 6.11); but in very dilute solution, the experimental measurements show a feature which is not reproduced by the theory. This is made clear by the theoretical (*a*) and experimental (*b*) curves shown by the *full* lines in the diagram. In this note, the effect responsible for the dip in the experimental curve in dilute solution is determined; its effect is shown by the dotted curve in the diagram. From this it can be seen that the new theoretical curve reproduces the shape of the experimental curve throughout the whole concentration-range.

In first deducing an unambiguous expression for the combinatory factor, and the various thermodynamical quantities of interest, it was shown[2] that the significant parameter was the number of closest neighbour interactions or contacts which one polymer molecule shares with its surroundings. For simple-chain and branched molecules, the number of closest neighbour interactions zq is given by

$$zq = zr - 2r + 2, \qquad (1)$$

where z is the co-ordination number of the quasi-crystalline model of a liquid and r is the number of segments in a polymer molecule. For very large values of r, appropriate to a high polymer, the ratio of the number of external contacts of a molecule to the number of sites occupied by it is almost exactly $z - 2$. This assumes, as is the case at ordinary



concentrations, that the polymer molecules are spread out and intertwined throughout the system.

There is evidence[2] that in dilute solution the polymer molecules tend to segregate, to coil up and to form clusters. My view has been (see ref. 1, sections 6.3 and 6.6) that the discrepancy, in the region of very dilute solution, between the theoretical and the experimental entropies was due to this change in the configurational properties of the individual polymer molecules as the concentration varied; that it did so through a reduction in the number of contacts of each polymer molecule; and that this gave rise to the observed variation of the heat of mixing of the rubber–benzene system.

Coiling of the polymer molecules in dilute solution implies physically that the number of external contacts is greatly reduced. This reduction will occur over a small concentration-range. Its effect can be seen in the following way. The entropy of dilution for a polymer-solvent system can be written, in terms of the parameters which define the polymer molecule, in the form

$$\frac{\Delta S_1}{R} = -\log(1-v_r) - \frac{r}{r-q}\log\left(1-\frac{r-q}{r}v_r\right). \quad (2)$$

(This is simply equation (4.24) of ref. 1, making use of equation (1) above.) If the right-hand side of this equation is expanded as a power series, the term in v_r vanishes; it is to this fact that the particular form of the entropy in such a system is due. Expanding the right-hand side, we get

$$\frac{\Delta S_1}{R} = \frac{1}{2}\frac{q}{r}v_r^2 + \frac{1}{3}\frac{q}{r}\left(2-\frac{q}{r}\right)v_r^3 + \ldots \quad (3)$$

Thus, the value of the entropy of dilution depends immediately on q/r, that is, on the ratio of the number of external contacts of a molecule to its number of segments. It is clear that if, as the system is diluted with solvent, the number of external contacts of a polymer molecule falls, owing to the fact that it coils up, then the entropy must assume a lower value than it would otherwise have. In fact, to a first approximation, the entropy will be lower than that calculated, assuming the fixed value of q given by equation (1), just in the ratio that the number of contacts has diminished. Assuming that the configurational properties of individual polymer molecules begin to change markedly at about $v_r = 0{\cdot}25$, corresponding to a molar concentration of the order of 10^{-3} or 10^{-4}, the theoretical curve now assumes the form shown by the dotted line in the diagram. The diagram shows that when this effect is allowed for, the theory reproduces the form of the experimental results completely.

The importance and usefulness of this concept of the number of contacts of a polymer molecule had been suggested previously[3,4] in other connexions; it clearly has other applications. For flexible polymer molecules there is self-blocking of some possible configurations owing to the polymer molecule bending back on itself[5,6]. This reduces the number of contacts of each polymer molecule; from the considerations which have been advanced in this note, it needs no elaboration to see that the effect must be very small. The same concept can be used also to consider systems of roughly spherical molecules which differ greatly in size, as is common in biological systems. In this case, the departure from ideal behaviour will be very much less than for a polymer–solvent system which has the same molecular volume ratios. The analysis given in this note also shows that there is no justification for suggesting[7] that the agreement between the experimental and theoretical free energy curves is due to a fortuitous cancelling of errors.

The configurational properties of the system, arising from the different shapes and sizes of the molecules of the polymer and of the solvent, are the dominating influence in the departure of polymer–solvent systems from ideal behaviour. The dependence on concentration of the configurational properties of individual polymer molecules can be adduced to account for the dip in the experimental curve for the entropy of dilution, which was not given by the basic theory used previously.

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Metastable States of Nickel

In view of the recent communication by Snoek and Fast[1] on their observation of the metastable states in *well-annealed* nickel, it may be of interest to record a similar effect which was noted in the course of some investigations on nickel magnetostrictive oscillators. These experiments, made just over a year ago, were essentially of a dynamic type, in contrast to the static conditions used by Snoek and Fast.

The specimen was a consolidated stack of laminations mounted on a pin at the central node, and it was excited at its resonant frequency (10·7 kc./s. at room temperature with zero polarizing field) by a beat-frequency oscillator applied to a driving coil closely encircling the stack. The E.M.F. induced by the motion of the stack in a screened 'pick-up' coil

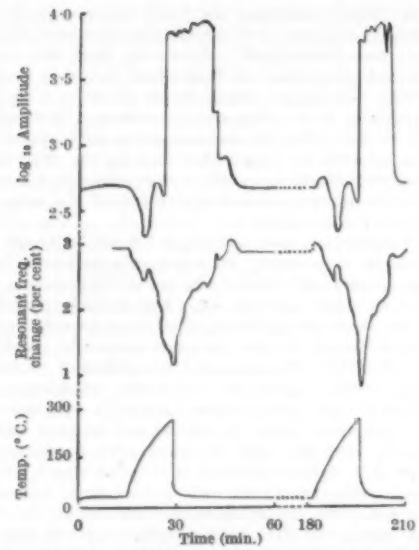

Fig. 1. Onset of metastable state with temperature rise (polarizing field 280 oersteds)

is proportional to the amplitude and also to the square of the dynamic magnetostriction constant, λ, as defined by the relation $\delta p = \lambda \delta H$, where δp is the incremental stress resulting from a small change δH in the applied magnetic field.

Magnetic polarization of the specimen was effected by passing a direct current (maximum value 7 amp.) through additional coaxial coils, the maximum polarizing field being 280 oersteds. At the higher currents used, considerable heat was developed in the polarizing coils, causing a rise in temperature of the stack. The surface temperature was measured by a contact thermocouple.

Typical curves are shown showing the variations with time in the amplitude of motion as the temperature varied, for the case of a laminated bar made from pure *cold-rolled* nickel sheet 0·005 in. thick. It will be seen that following a rapid rise of temperature (brought about by shutting off the forced-air cooling

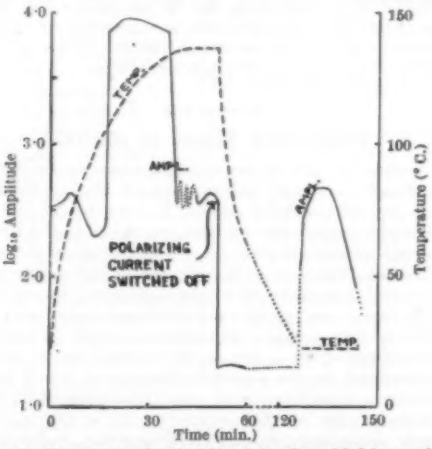

Fig. 2. Recurrence of metastable state after nickel has cooled (polarizing field 200 oersteds)

system), there is a large increase in λ, which it is possible to maintain for a period of hours or even days. This effect was not obtained with similar experiments made on a number of nickel alloys.

It may be found that these metastable conditions can be employed for a highly efficient magnetostrictive transducer, which could be used in special circumstances where the higher temperatures do not present any difficulties.

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Decay of Thulium-170 and Rhenium-186

THE decay of the 127-day thulium isotope (Tm^{170}) has been previously investigated by Bothe[1], and that of the 93-hour rhenium isotope (Re^{186}) by Goodman and Pool[2]. In both cases absorption methods were used.

The present investigations were carried out by means of absorption and coincidence techniques, and also by means of a short magnetic-lens β-ray spectrometer.

A summary of our results appears below:

	β-rays	γ-rays
Tm^{170}	1·00 ± 0·01 MeV. 0·90 ± 0·015 0·79 ± 0·03 0·45 ± 0·05	82·6 ± 0·7 keV. 200 ± 10 440 ± 20
Re^{186}	1·095 ± 0·010 MeV. 0·945 ± 0·015 0·64 ± 0·030	132 ± 1 keV. 275 ± 5

A more detailed account of this work will appear shortly.

We wish to express our appreciation of the help given by members of the Atomic Energy Research Establishment, Harwell, where the active samples were prepared by slow neutron irradiation in the low-energy pile.

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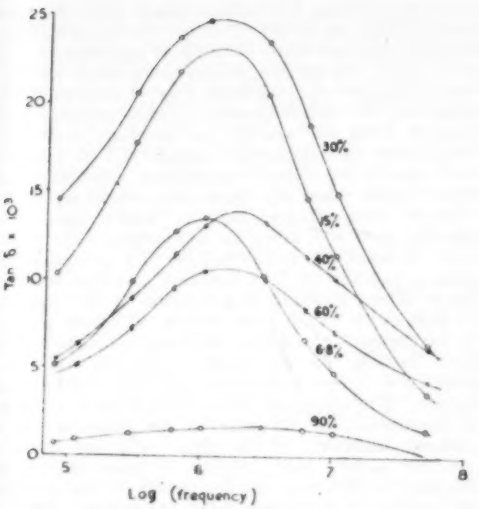
Dielectric Absorption in Crystalline Long-Chain Ketones

FOR dilute solid solutions of long-chain polar compounds in paraffins, it has been shown that the rotational transitions of the polar molecules from one position of minimum potential energy to another lead to dielectric absorption of the Debye type[1,2]. A theoretical study has also been made[3,4] of the dielectric properties of pure crystalline compounds, in which dipolar interaction is an important factor.



Dielectric absorption at 20° C. for a range of concentrations of laurone in *n*-hexacosane

In order to investigate the effects of dipolar interaction, it seemed desirable to make dielectric measurements with mixtures containing a gradually increasing proportion of polar molecules. The accompanying graph shows the frequency / tan δ relationships for a series of concentrations of the ketone, laurone ($C_{23}H_{46}O$), in a non-polar solvent, *n*-hexacosane. The measurements were made at 20° C., and cover the frequency-range, 60 kc./s. to 50 Mc./s. It is seen that the dielectric loss at first increases with increasing concentration but later decreases, pure laurone giving negligible loss within this range.

With concentrations up to at least 15 per cent, the majority of the laurone molecules are probably surrounded by non-polar solvent. The rapid increase of the maximum tan δ with concentration in this range suggests that solute molecules thus situated are able to perform rotational transitions leading to dielectric absorption in an alternating electric field. A small proportion of the ketone molecules, however, occupy adjacent positions in the crystal lattice, and the resulting dipole interaction probably causes a very high energy difference, V, between equilibrium positions. The probability of finding such molecules in a higher equilibrium position is thus small, and they therefore contribute little to the polarization. As the concentration of laurone increases, the proportion of adjacent molecules increases rapidly, the effect on the polarization being indicated by the dielectric absorption results given. It is probable that pure crystalline laurone provides an example of the type treated theoretically by Fröhlich[5], in which, even at room temperature, $kT \ll V$, and there is no dielectric loss of the Debye type. Similar results were obtained with the ketones, myristone ($C_{27}H_{54}O$), palmitone ($C_{31}H_{62}O$), stearone ($C_{35}H_{70}O$) and 22-tritetracontanone ($C_{43}H_{86}O$).

In contrast to the ketones, several long-chain esters gave strong absorption of the Debye type. The maximum tan δ and frequency values for three such compounds are given in the accompanying table.

Ester	Tan δ (max.)	Frequency (max.)
Dodecyl laurate, $C_{24}H_{48}O_2$	$1{\cdot}4 \times 10^{-2}$	3 Mc./s.
Dodecyl myristate, $C_{26}H_{52}O_2$	$4{\cdot}0 \times 10^{-2}$	600 kc./s.
Cetyl palmitate, $C_{32}H_{64}O_2$	$1{\cdot}5 \times 10^{-2}$	100 kc./s.

Thus, with these esters, the energy difference between equilibrium positions is probably less than in the ketones of corresponding chain-length, and a significant proportion of the molecules are to be found in the second position. This suggests that dipole interaction in the esters is less than in the corresponding ketones.

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Potassium and Neuromuscular Transmission

In a study of the effects of ions on the rat diaphragm, it has been found that denervated preparations are much more sensitive to potassium than the normal diaphragm stimulated through its nerve. A dose of potassium, for example, exposure to a concentration of 0·08 per cent in Krebs's solution, which has no depressant action on normal muscle, may completely depress the response of denervated muscle to electrical stimuli while its response to acetylcholine remains. Observations have now been made on diaphragms denervated functionally by curare, which prevents the action of acetylcholine, or by lack of glucose, which prevents its synthesis or release. The effect is the same in both cases. If now, however, the experiment is reversed and the potassium added first, it is found that after curare, or through lack of glucose, which abolish neuromuscular transmission, the muscle has become inexcitable. The exact cause of this somewhat dramatic finding is not certain.

An obvious suggested explanation of these facts might be that potassium and curare have a synergic action, or that acetylcholine and potassium oppose each other. Another explanation would be that the potassium does, in fact, render the muscle inexcitable to direct stimulation in all cases, but it can remain excitable to nerve stimulation only. This explanation is supported by preliminary observation that, although after potassium the muscle contracts normally, its resting potential is reduced by one third, as shown also by Boyle and Conway[1], and its action potential is negligible. This latter observation is in accordance with that of Brown and Euler[2], that action potential may be reduced by potassium while the muscle tension is increased.

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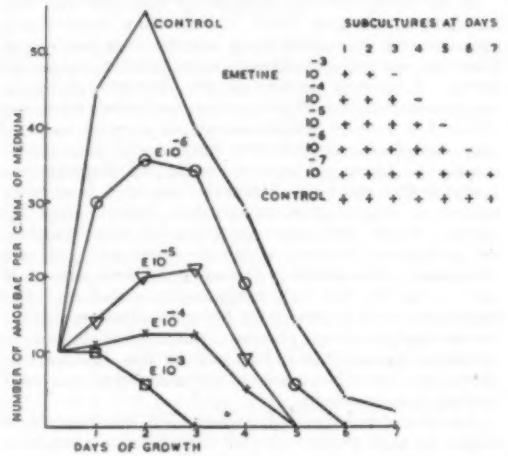
Nature of the Action of Emetine upon *Entamœba histolytica*

THE fact that emetine exerts a powerful toxic action upon *E. histolytica*, *in vivo* and *in vitro*, has long been established[1,2]. The mechanism of this action, however, is far from clear[3]. Furthermore, the end-point of toxicity of emetine to *E. histolytica* is variable *in vitro*, depending upon such factors as the nature and reaction of the medium, time of observation, and composition of the bacterial flora which necessarily accompanies the growth of the amœba in culture[3,4].

In the experiments reported below, emetine hydrochloride was added (*a*) to cultures of *E. histolytica* in a basic medium of horse serum, rice starch and saline buffered with phosphate to *p*H 7·2, and (*b*) to suspensions, prepared from such cultures, with a cell density of 150–300 per c.mm. Ten strains of *E. histolytica* were used, each having been isolated originally from cysts in human fæces. One strain (*M*) was grown with *Bact. coli* plus *Cl. welchii*; the other strains included in their bacterial flora non-lactose-fermenting coliforms, *Ps. pyocyaneus*, enterococci and *Clostridia*. Acting in the basic medium, emetine did not inhibit the growth or respiration of these bacteria, individually or in combination.



At *p*H 7·2, emetine (in dilutions ranging from 10^{-6} to 10^{-7}) sterilized cultures of *E. histolytica* in three to seven days. A study of growth curves showed that this effect was due to a reduction of the propagation-rate (see graph). No division occurred at dilutions of 10^{-3} to 10^{-4}; at higher dilutions there was a progressive increase in the rate of division, and dilutions higher than 10^{-7} showed no difference from control cultures. The addition of cysteine (0·02 per cent) did not antagonize the action of emetine.

When emetine was added to suspensions of *E. histolytica*, there was no change in the morphology of the cells, and subculture of washed cells was always positive during the first four hours. Thereafter, at dilutions of 10^{-3} or less, emetine produced conspicuous degenerative changes in 80–100 per cent of the cells, clearly shown in fresh or iron–hæmatoxylin preparations. Bi-nucleate or dividing cells were absent, and, after 48 hours, subculture was negative. With higher dilutions, there was a proportionate decrease in the number of degenerate cells; dividing cells were present and subcultures made after 48 hours were positive, as in the controls. The morphological changes observed were, in order of appearance: loss of motility, vacuolation, coarse granulation and central retraction of cytoplasm, loss of nuclear differentiation, and disintegration. These morphological changes were not specific for emetine, since cells comparable in appearance though not in numbers were seen in the control suspensions, especially after 24–48 hours, when the death-rate rose sharply.

From these observations it would seem that emetine does not exert an immediate toxic action upon *E. histolytica* in the fashion of a general protoplasmic poison. It does, however, have an 'amœbostatic' effect, absolute at a dilution of 10^{-3} but still evident at 10^{-7} with some strains. Cultures growing in the presence of emetine are sterilized more rapidly than controls for the reason that the cell count reaches a lower maximum, and the culture dies sooner. The end-point of survival of such a culture is determined by the ratio of propagation-rate to natural death-rate of the cells. The cell envelope remains intact, and motility is preserved in the majority of the cells for four hours in the presence of emetine. Surviving cells, transferred from an emetine medium after 24–48 hours, grow normally. Emetine does not produce a definite stage of mitotic arrest, and its action is not antagonized by cysteine, an —SH donor. It seems likely, therefore, that emetine acts upon *E. histolytica* by a highly specific interference, in proportion to its concentration, with some essential cytoplasmic reaction at a phase in the growth of the cell prior to cell division.

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Effect of the *Striga* Germination Stimulant on Extension Growth on the Roots of Peas

THE seeds of *Striga hermonthica*, like those of certain closely allied parasites, will normally only germinate after they have been treated with a stimulant that is released from the roots of a large variety of species[1]. The situation suggests that the parasite seed is supplied from actively growing host roots with a hormone which is normally required in germination but which is not synthesized by the seed[2]. Moreover, the hormone only acts at a comparatively late stage in the germination process, and then it has the effect of promoting the extension of the cells of the radicle. When the radicle emerges, at first there is no division at the apex, and the increase in length is apparently due primarily to cell extension. Thus the hormone may be one that promotes cell extension in the root of *Striga*, and since it is produced from a large variety of species it may be one that stimulates extension growth in roots in general[3].

This possibility has been investigated by treating segments of pea roots taken from the extending zone with various dilutions of a solution of a concentrate of the hormone. The segments are obtained by cutting out of intact roots the zone extending from 1·5 to 3·0 mm. from the tip. Observations can be made rapidly with this material, and extension

growths in the root can be studied independently of the tip, both considerations being of importance since the available evidence suggests that the hormone is produced in the tip and that it is highly labile. In the normal procedure, the segments after they are excised are placed on sintered glass disks standing in the experimental solutions, twenty segments being allocated to each disk. After incubating for 18 hr. at 25° C. in the dark, the segments are examined microscopically, and the length and breadth of each is measured against a calibrated micrometer eyepiece. Each figure given below is the mean of twenty observations made with each treatment.

The mean lengths and breadths (mm.) given with various dilutions of the concentrate and with water are presented in Table 1, which also includes the calculated percentage increases in volume over the original volume at the beginning of the experiment. It will be noticed that at a concentration of 10 mgm./litre the preparation of the natural stimulant gives a volume increase considerably greater than that of the control with distilled water (in Table 1 the differences in the group of means for breadth are not significant, but those for length are highly significant).

Table 1

Concentration (mgm./litre)	Length	Breadth	Percentage increase in volume
0 (Control)	2·17	1·10	55·9
100	2·25	1·10	61·5
10	2·35	1·13	81·1
1	2·12	1·11	57·8
0·1	2·14	1·10	53·7
0·01	2·15	1·11	60·0

The stimulant involved in the germination of the *Striga* seeds has not been isolated in the pure state; but it has been found that concentrates of the natural stimulant that give germination at high dilutions contain pentose sugars which are probably not aldopentoses, and that D-xyloketose may induce germination at lower concentrations than the purest preparations of the natural stimulant that have so far been made[3]. Thus it is probable that the natural stimulant is at least similar to D-xyloketose, and the effect of this sugar on the extension of pea root segments has therefore been examined in some detail. The results of two experiments with xyloketose conducted with the technique described above are shown in Table 2. It is evident that at appropriate concentrations of D-xyloketose, increases in length and breadth, and therefore of volume, are given that are considerably greater than the corresponding increases given with water. (The differences in the means for length are highly significant for both *A* and *B*; for breadth they are not significant for *A* but highly significant for *B*.) Moreover, it may be noted that stimulation of extension growth is given with concentrations so low as 0·1 mgm./litre.

Table 2

Concentration (mgm./litre)	Length		Breadth		Percentage increase in volume	
	A	*B*	*A*	*B*	*A*	*B*
0 (Control)	2·33	2·15	1·08	1·10	65·1	51·6
1000	2·42	2·34	1·07	1·15	70·9	79·1
100	2·39	2·22	1·08	1·14	70·7	68·6
10	2·48	2·37	1·09	1·15	80·5	83·3
1	2·59	2·30	1·12	1·13	99·1	71·6
0·1	2·38	2·22	1·09	1·10	73·9	56·8
0·01	2·31	2·20	1·09	1·10	67·3	55·9

All results obtained with the natural stimulant have shown some increase in breadth at certain concentrations of the stimulating solution. In certain instances the increases in breadth are not statistically significant; but they have been observed repeatedly, and they are a characteristic of the stimulation due to this hormone. With pea-root segments, heteroauxin may give a slight increase (less than with D-xyloketose) in length at a concentration of 0·001 mgm./litre, but increases in breadth have never been observed. Indeed, the breadths of the treated segments are always lower than those of the water controls for all concentrations of heteroauxin greater than 0·0001 mgm./litre. The differences between the volumes of the segments treated with heteroauxin and those of the water controls are rarely as great as 10 per cent, whereas the corresponding differences between those treated with D-xyloketose and the controls are rarely less than 30 per cent, and may be as high as 60 per cent at the appropriate concentrations.

Finally, so far as we know, this is the first record of a stimulating effect on the growth of plant tissues of a simple sugar in concentrations of less than 10 mgm./litre.

We wish to express our thanks to the Colonial Products Research Council for financial support in this investigation, and to Mr. A. G. Long for preparing some of the samples of D-xyloketose that have been used.

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Free Amino-acids in the Hæmolymph of *Calliphora erythrocephala* Meigen

The recent publication by Raper and Shaw of an analysis of the amino-acids of the hæmolymph of the nymph of *Aeschna cyanea*[1] prompts us to report a similar investigation we have carried out upon the hæmolymph of the larva of *Calliphora erythrocephala*. The larvæ used for this study were inbred descendants of a single female and were reared on ox liver at 25° C. for 4–5 days. The amino-acids were separated and identified by paper partition chromatography.

About fifteen larvæ were washed with water and then each larva was bled on to a small strip of filter paper by puncturing the heart with a needle. These papers were dropped into 90 per cent alcohol, and so a clear solution of the free amino-acids was obtained; the protein constituents remained adsorbed on the filter paper. This solution was concentrated to about 0·15 ml. on a water-bath or, preferably, at room temperature *in vacuo*, and then 20-c.mm. samples of this were used for analysis. One- or two-dimensional chromatographic analysis was carried out by the ascending technique[2] using phenol ammonia, *n*-butanol-acetic acid (4/1, by volume), collidine–lutidine (1/1, by volume) as solvents. The papers were dried and finally developed by spraying with ninhydrin. The accompanying table lists the amino-

Amino-acids	Proportion	Amino-acids	Proportion
Alanine	2·5	Phenylalanine	0·5
Tyrosine	2·0	Glycine	0·3
Proline	1·0	Leucine	0·3
Valine	1·0	Lysine	0·2
*iso*Leucine	0·7	Serine	0·2
Histidine	0·5	Aspartic acid	0·1

acids identified and gives an estimate of their relative proportions by weight.

There are several interesting features about the results of this analysis. Phenylalanine has not previously been found in hæmolymph in other larvæ, for example, *Dytiscus marginalis*[2], *Oryctes nasicornis*, *Melolontha vulgaris*[4] or *Aeschna cyanea*[1]; but in this case it occurred in appreciable proportion. It is of interest to note that Goldberg and de Meillon have recently shown that phenylalanine is essential for the growth of the larvæ of *Aedes ægypti*[5]. The same workers showed that cystine and methionine were necessary constituents of the diet. It seems surprising that neither of these amino-acids has, as yet, been detected in any hæmolymph.

Florkin and Duchateau[3] have reported the tyrosine concentration in the hæmolymph of *Dytiscus marginalis* as 117–168 mgm. per cent. Taking their value of 106 mgm. per cent amino-acid nitrogen, our results would correspond to about 150 mgm. per cent tyrosine, a value comparable with their result. Such a concentration seems abnormally high when compared with the solubility of tyrosine in water (45 mgm. per cent at 25° C.). The proportion of tyrosine reported in our table was confirmed by direct comparison with the valine using spot-dilution technique.

The presence of both leucine and *iso*leucine was shown by separation of the 'leucine fraction' by one-dimensional chromatography; then, after extraction from the paper, the two were separated by further chromatography using benzyl alcohol. The trace of histidine was detected by treating the appropriate region of the paper with diazotized sulphanilic acid in sodium carbonate solution. Histidine then shows as an orange-red coloured spot.

This work is part of an investigation of the nutrition of tissues. It is hoped to study the metabolism of both insect and mammalian tissues *in vitro*.

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Use of Wetting Agents against Heath and Undergrowth Fires

Suggestions have been made[1] that by the addition of wetting agents an economy can be achieved in the use of water in fire fighting, particularly with heath and undergrowth fires. Preliminary experiments which we have recently completed indicate some of the probable limitations of this proposal, and also suggest fields in which it might have some practical use. The combustible materials used were ling (*Calluna vulgaris*), bracken (*Pteridium aquilinum*) and purple moor grass (*Molinia cærulea*); the samples were provided by the Forestry Commission at the end of the winter and after a long dry spell, so that they were in their most dangerous condition from the point of view of the forest fire-fighter.

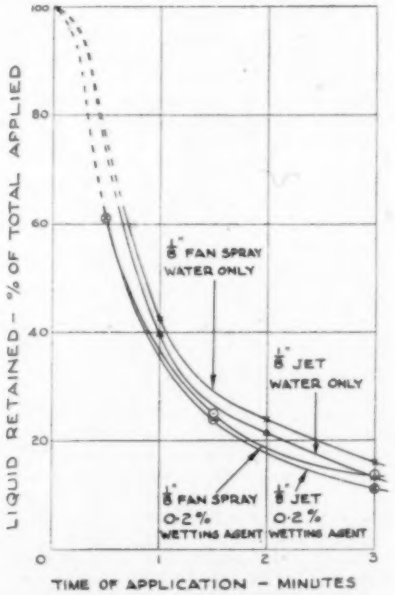
Three series of experiments were carried out: (i) extinction of fires; (ii) prevention of spread of fire; and (iii) wetting of dry materials at normal temperature (that is, without fire).

In each series a comparison was made between the effectiveness of water and that of a solution of each of three commercial wetting agents (one non-ionic and two anionic), and of application in the form of a jet and as a spray. The wetting agents were used in 0·2 per cent aqueous solution, since increases in concentration beyond this point did not greatly reduce the surface tension with any of the agents used.

For the fire extinction experiments, the material was spread on a circular area of ground, and was ignited at the centre. The fire was attacked when its diameter was approximately half that of the circular area. No significant difference was observed between water and the wetting agent solution, either in the time taken to extinguish the fire or in the amount of liquid used.

For the second series of experiments, a 6-in. layer of *Molinia* was spread in a rectangular colander, 4 ft. long × 2 ft. wide. Liquid was applied by means of a spray for a predetermined period to one half of the specimen, the other half being covered. At the end of the wetting period, the cover was removed from the dry half, which was then ignited. It was found that the quantity of liquid required to prevent the advance of flame from the dry to the wet zone was half as much with the wetting agent solution as with water.

In the third series of experiments, a 6-in. layer of each of the materials investigated was placed in the colander, and liquid was applied for a given period as either jet or a spray, and the quantity retained in the sample was determined by difference in weight before and after application. Typical



Retention of liquid in samples of ling

results with ling are shown in the accompanying graph; little difference was revealed between the behaviour of wetting-agent solutions and water.

Within the scale of the experiments it was apparent that, though wetting agents gave no advantage in fire extinction, they may well prove to be of value in controlling heath fires by the technique of wetting the unburnt materials ahead of the fire. This is reasonable in view of the fact that, whereas the small quantities of wetting agent used can have little effect upon the thermal properties of the water, the reduction in surface tension which they bring about is likely to result in improved dispersion within the combustible material even when it causes no appreciable change in the total quantity of liquid retained.

It is intended to examine further the efficacy of wetting agents in similar materials of different specific surface.

This letter is published by permission of the Director of Fire Research.

J. F. Fry
P. M. T. Smart
Department of Scientific and Industrial Research,
and
Fire Offices Committee,
Joint Fire Research Organisation,
Boreham Wood, Elstree, Herts.
Dec. 4.

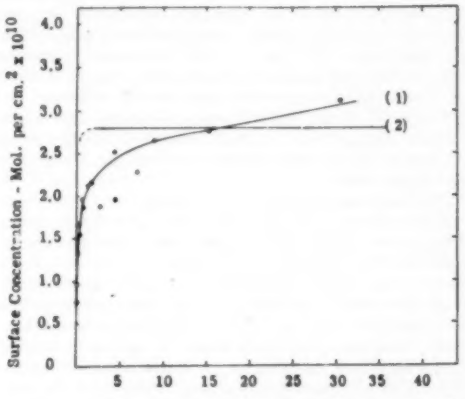
[1] For example, Edson and Parker, *Fire Engineering* (August, 1947)

Measurement of the Adsorption of Surface-Active Agent at a Solution–Air Interface by a Radiotracer Method

The adsorption of a water-soluble surface-active agent at a solution – air interface has been measured by taking unique advantage of the soft beta radiation from the radioactive isotope of sulphur, S^{35}. The method depends upon the fact that, because of 'self-absorption' of the radiation by the solution, the amount of radiation escaping from a solution of a compound containing this isotope comes principally from that fraction of the sulphur which is near the surface. Since the molecules of a surface-active agent are preferentially adsorbed at the interface, the radioactivity above a solution containing such an agent labelled with radiosulphur should be higher than the activity from a solution (for example, of sodium sulphate) in which no surface adsorption occurs.

To investigate this idea experimentally, di-*n*-octyl sodium sulphosuccinate ('Aerosol OTN') and sodium sulphate were each synthesized with sulphur-35 (procured from the U.S. Atomic Energy Commission). A pure 2–3-mgm. sample of the surface-active agent was secured by five recrystallizations from alcohol – water solutions. Activity measurements were made by placing a mica-window bell-type Geiger counter over small open pans containing the solutions. Measurements were carried out on solutions of the sulphosuccinate and of sodium sulphate having identical concentrations and specific activities. Since the sulphate is not adsorbed at the interface, a comparison of the radioactivity of its solution with that of the surface-active agent permitted a calculation of the amount of agent adsorbed in the interface.

The experimental results showed that the radiotracer technique permitted a successful quantitative determination of the adsorption. Depending upon the specific activities and the concentrations, the counts per minute (c./m.) over the solutions of agent were 1·1 to 20 times those of the corresponding sodium sulphate. For example, in one case the sulphosuccinate solution showed 2,360 c./m., the sulphate solution only 323 c./m. Measurements of some thirty-five different solutions covering a range of specific activities (1×10^{12} to 5×10^{9} counts/min./mole) and of concentrations ($0{\cdot}1$ to 150×10^{-6} moles/c.c.) produced a well-defined adsorption isotherm. It is shown in the accompanying graph, along with an isotherm calculated by the conventional Gibbs equation from surface tension measurements covering the concentration range 4 to 50×10^{-6} moles/c.c. The radioactivity measurements suggested that, after the agent had formed a unimolecular layer, a further increase in bulk concentration caused further adsorption at the interface.



Adsorption isotherm for aqueous solutions of di-*n*-octyl sodium sulphosuccinate. The radioactive isotherm, Curve No. 1, continues without change of slope to a concentration of 150×10^{-6} mol. per c.c., at which point a surface concentration of $5{\cdot}5 \times 10^{-10}$ mol. per cm.² is indicated. The surface tension isotherm, Curve No. 2, remains horizontal to the critical concentration (65×10^{-6} mol. per c.c.). (Temperature, 24° C.)

An interesting aspect is that the rate of adsorption was followed in the low-concentration ranges. The method promises to be useful for studying the kinetics of such adsorption processes.

The radiotracer method outlined above seems to be generally applicable to surface phenomena involving adsorbed layers, either mono- or multi-molecular in nature. Although this note describes the use of sulphur-35, it is evident that other radio-isotopes having sufficiently soft radiation, such as carbon-14, calcium-45, iron-59, etc., could be employed. Moreover, the method appears to open considerable possibility for examining interaction in surface layers. For example, by using labelled sulphate, the adsorption of sulphate ions in the surface layer in a solution containing both cetyl pyridinium chloride and sodium sulphate has already been measured. The application of the method to the study of surface phenomena in bubbles and in systems containing several surface-active materials is under consideration.

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A. J. Weith, jun.
A. A. Argyle
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Feb. 21.

Quantitative Chromatographic Analysis of Tetra-, Tri- and Di-methyl Fructoses

In a recent report by Gilbert and Stacey[1] on the constitution of a bacterial levan, these authors state that a convenient chromatographic technique for the quantitative analysis of tetra-, tri- and di-methyl fructoses had so far eluded them. We have devised a partition chromatogram giving quantitative results in the analysis of mixtures of 1 : 3 : 4 : 6-tetramethyl, 1 : 3 : 4-trimethyl and 3 : 4-dimethyl fructoses which is applicable to the study of methylated fructosans based on these sugars. (3 : 4 : 6-Trimethyl fructose has also been examined; but certain difficulties, not yet overcome, arise in analytical separations involving this sugar.)

In principle, the method resembles the separation of methylated glucoses devised by one of us[2], but it differs in practice in two important respects. First, the partitions of the sugars concerned between organic solvents and water necessitate use of a different series of solvents for the mobile phase; and secondly, 1 : 3 : 4 : 6-tetramethyl fructose has appreciable volatility at reduced pressures, both in the dry state and in concentrated solutions, but not in dilute solutions.

The essential steps in our method are as follows. (1) The solution of the sugars is extracted ten times with its own volume of chloroform. The chloroform phase is concentrated (bath temp. 30°) to 50 ml. (no loss occurs). The chloroform is then displaced by distillation with toluene at low temperature (final volume about 50 ml.). This is pipetted on to a silica – water column prepared from a slurry of silica, containing half its weight of water, and toluene.

(2) The tetramethyl fructose is eluted by passing sixteen column-lengths of toluene containing 0·33 per cent of ethanol. The eluate is concentrated (30° C.) to 50 ml. and extracted eighteen times with 50-ml. lots of water, and the combined water phases made up to a known volume. The sugar is then estimated colorimetrically, taking an aliquot of the water phase containing approximately 0·4 mgm.

(3) The water phase from (1) is evaporated to dryness (30° C.), the residue dissolved in a little chloroform and this solution pipetted on to the column. The total trimethyl fructose is then eluted by means of eight column-lengths of chloroform containing 5 per cent (v/v) of *n*-butanol. The eluate is evaporated to dryness (30° C.), some water being added to aid removal of the butanol. The residue is dissolved in ether and this solution evaporated to dryness in a tared evaporation-flask[3]. The dried sugar is then weighed.

(4) Dimethyl sugars are extracted from the column by methanol. This solvent is evaporated, and an ethyl acetate solution of the residue is evaporated and weighed as for the trimethyl sugar.

Recoveries for all three sugars are of the order of 98 per cent (scale of working 100–1,500 mgm.). In every instance the sugars were shown by paper chromatography[3], optical activity and analysis to be pure. A full account of this work is in preparation.

D. J. Bell
A. Palmer

Biochemical Laboratory,
Cambridge.
Dec. 1.

[1] Gilbert, V. E., and Stacey, M., *J. Chem. Soc.*, 1560 (1948).
[2] Bell, D. J., *J. Chem Soc.*, 473 (1944).
[3] Brown, F., Hirst, E. L., Hough, L., Jones, J. K. N., and Wadman, H., *Nature*, **161**, 720 (1948).

The Teaching of Biology in Schools

I was surprised at the warmth of the hostility with which Prof. Graham Cannon reviewed my pamphlet "Social Biology for Sixth Forms" in *Nature* of April 9, p. 577.

The problem which my paper attempts to answer is this. Supposing that a headmaster will allow a biologist two periods a week to lecture to sixth-form boys specializing in history, classics, mathematics or modern languages, what should he attempt? In my view the answer to this question must take account of three things.

First, the interests of these boys are mature interests. They want to put the questions to a biologist that an adult would ask. Secondly, if biology is worth a place in their education at all, it is because it has implications which affect their philosophy, politics and understanding of history and world problems of the day, such as soil erosion or food supplies. Thirdly, as anybody who knows sixth-form time-tables must be aware, it is unrealistic to expect more than two or three periods a week for such a course, and the time must be carefully used. If the boys have not done any practical biology at an earlier stage—and I should agree with Prof. Cannon in deploring this—it is useless to start here, and unlikely to impress them with the importance of the subject. Of course, one cannot deal exhaustively with the many applications of biology to human affairs in the time available; but one can, as I have shown in practice, arouse interest in the work of biologists which bears on social problems, indicate the kind of answers they would give, and send the boys out keen enough to follow up the work by reading for themselves, and free from some of the superstitions and prejudices with which otherwise reasonable people sometimes approach psychology, heredity, evolution, the birth-rate, intelligence-tests and so on.

Prof. Cannon attacks my course first on the ground that it covers too large a field, and then objects strongly to the idea of selecting some aspects for more detailed treatment than others. The idea that the interests of the class should be a guide as to which problems one pursues comes as a novelty to him, apparently, as in his opinion the order of the course (as laid down by T. H. Huxley, significantly!) must not be disturbed. Later in his review it becomes clear that his eyes are really on examinations rather than education, and, of course, the need to adhere rigidly to a given syllabus is one of the few major drawbacks to an examination system.

The criticism that a course of biology should be based on adequate practical work, natural history and comparative anatomy and embryology is, of course, unanswerable when the biology students, with whom Prof. Cannon deals, are under consideration; but completely impracticable for the history and classical scholars with whom, one suspects, he has very little contact. A good deal can be done by demonstration; they are shown the chromosomes, and the look of an F_2 generation of *Drosophila*, or plankton samples, and they value the experience. But to ask that they should learn the technique of *Drosophila* culture, or that they should be provided with microscopes and personally record the various links in the food-chain of the herring, is unrealistic for the non-specialist.

I should also deny that a course of this kind encourages boys to learn "whole passages or whole

problems by heart on the chance of getting a question on them in the examination". In practice it is designed to make them think, and relate theoretical and applied biology to the problems of their time and to their other school subjects. I do not, of course, claim that my outline syllabus is the only answer, or necessarily the right one. It is the result of only fourteen years of experiment in one school. But I am sure that something of the sort is well worth while, and that professors of any of the biological sciences could help by making constructive suggestions as to what scientific ideas the future Civil Servants, lawyers, industrial leaders or clergy of Great Britain should be made aware of. It must be remembered that most of these will come from the sixth forms of our grammar schools, and that for many of them a course of this kind will be the last formal science teaching they will receive. Prof. Cannon does not, however, make constructive proposals.

The accusation that there is too much emphasis on what Prof. Cannon calls 'Neo-Mendelism' is based on a misreading of my paper. The second term's work is not "devoted very largely to a study of Mendelism", but centres on evolution, and embraces also the study of population growth, variation and environmental influences and extinction. When Prof. Cannon writes, of Mendelism, "It is easy to teach, that is certain", I leave it to those who have experience of teaching even highly intelligent non-specialists to judge whether my course or Prof. Cannon's review is the more 'fantastic'.

ERIC LUCAS

The College,
Winchester.
April 25.

MR. LUCAS in his original pamphlet is emphatic that his proposed syllabus is for sixth-form boys specializing in classics, history, mathematics: in fact, in anything other than natural science. Now in the third paragraph of his letter above, he says that if these boys have "not done any practical biology at an earlier stage . . . it is useless to start here". I can only take this to mean that unless these arts students have done some elementary biology in their earlier years at school and so done some *practical* biology, his syllabus for the course of study at the sixth-form level is useless. Is he assuming, therefore, that in these enlightened days all boys, whatever subject they may ultimately specialize in, have had a groundwork in the elements of biology in their early school days? This may be so at Winchester; but does he not know that, in the greater number of secondary grammar schools in which biology is taught at all, it is only taught on the science side, and in many of these schools is only dealt with in the sixth form? His pamphlet, as I pointed out, "purports to deal with the way in which biology and social biology at that can be introduced to a sixth form who have done no biology before"—not the sixth form of any particular school but presumably at any secondary grammar school. That is the way in which I maintain his pamphlet will be interpreted and that is why I criticize it.

But also perhaps I have been guilty of inaccuracy in my review because Mr. Lucas seems to have deduced that I would insist on practical work in the type of biology course he is considering. I did not, however, say this. I merely pointed out that a university professor had the advantage of adequate time for practical work guided by a bevy of demonstrators. If time is short (and nobody disputes this as regards Mr. Lucas's course) then practical work is the last thing I should insist upon. Why, almost as much time can be wasted over practical work in biology as over field-work in botany! By all means have as many demonstrations as possible, as Mr. Lucas suggests, but heaven forbid that I should ever be called upon to demonstrate an F_2 generation of *Drosophila* to any class, sixth form or otherwise, let alone teach these same boys how to culture the annoying little creatures. No, if I had two periods a week for one year at my disposal and an intelligent sixth form, then, always remembering that the boys had done no biology before and also that the time must be carefully used and not frittered away in snippets here and snippets there, I feel that I could use that time profitably by talking about the *elementary* facts of biology—giving the students some idea of what an organism is, particularly what a human organism is and its relation to its surroundings. This would then enable these boys later on to develop their ideas on these foundations, to ask themselves and others questions that were of importance and not mere trivialities. Mr. Lucas says that his sixth-form boys "want to put the questions to a biologist that an adult would ask". They may want to: but are they equipped to do so? When they have been given the foundations of the subject they certainly can, if they are intelligent, ask valuable questions; but until then it will be more by luck than judgment if they manage to avoid the type of inanity with which we are so familiar in our first-year students—and, after all, our first-year students are only glorified sixth-formers. That is why, by the way, I may not be so unfamiliar with the type of student that Mr. Lucas deals with as he appears to imagine.

Then he reproaches me with having an eye on examinations—but why not? I have had plenty of experience of how schools *do* cater for examinations—especially those for open university scholarships. We all know of the irrational scramble for scholarships among schools, and this all involves catering for examinations: so why not assume that this sixth-form 'culture' is encouraged with one eye on the examinations? What about this general knowledge paper which it is proposed to introduce into the new examination? Mr. Lucas's syllabus is just the sort of thing for this paper: and who suggested it?—Was it the schools or the examining boards? It was certainly not the latter in this part of the country.

Finally, I am sorry that Mr. Lucas finds genetics a difficult subject to teach. I can only repeat what I said at the meeting of the British Association at Brighton last year, referring to this aspect of biology, "despite the profundities of the Neo-Mendelians, it is still possible to deal with this matter adequately in a few lectures".

But why should Mr. Lucas worry over such a trifle? As his original pamphlet points out—if a subject is too difficult for the student or the teacher, just drop it and go on to something more entertaining.

H. GRAHAM CANNON

Department of Zoology,
University of Manchester.

INELASTIC SCATTERING OF PROTONS BY MAGNESIUM AND ALUMINIUM

By E. H. RHODERICK
Cavendish Laboratory, Cambridge*

THE inelastic scattering of protons by some light elements has been investigated experimentally, using 4·5-MeV. protons from the Cambridge cyclotron. The energy spectrum of the scattered protons was determined by using a proportional counter in conjunction with absorbing foils. The experimental arrangement was similar to that used by Dicke and Marshall[1], except that provision was made for observation of the scattered protons at angles between 20° and 160° at 10° intervals. Only proton groups corresponding to excited states less than 2·5 MeV. above the ground-state were able to reach the counter. The resolving power was improved by a factor of four over that obtained by Dicke and Marshall by the simple expedient of rotating the scattering foil through 90° from the position adopted by them, which minimizes the effect of target thickness. Under these conditions the resolving power was about 250 keV. for inelastic groups of equal amplitude.

The spectrum of the protons scattered by magnesium is shown in Fig. 1. The first inelastic group corresponds to a Q value of $-1{\cdot}36 \pm 0{\cdot}03$ MeV., and in view of its large amplitude is almost certainly associated with the predominant isotope, magnesium-24. The excited state of the residual nucleus which is involved is probably that which is responsible for the 1·38-MeV. gamma-ray emitted following the beta-decay of sodium-24, which has been measured very accurately by Siegbahn[2]. The Q value of this group remained constant with angle to within $\pm 0{\cdot}015$ MeV., which represents the relative error in the measurements at different angles. The second inelastic group corresponds to an excited state $1{\cdot}82 \pm 0{\cdot}04$ MeV. above the ground-state, and in view of its small amplitude may belong to an isotope other than magnesium-24. The presence of an inelastic group at this energy has recently been confirmed by Bush and Fulbright[3], though these authors state that its amplitude is comparable with that of the first inelastic group for higher bombarding energy.

The spectra of the protons scattered through three different angles by aluminium are shown in Fig. 2, from which it is seen that the first inelastic group has a complex structure. At 90° the peak is nearly resolved into two groups, and can be well explained by the existence of excited states in aluminium-27 respectively 0·80 and 0·97 MeV. above the ground-state. The shape of the peak varies considerably with angle, and in general is too indefinite to admit of such a precise interpretation as that given at 90°, though the left-hand component seems fairly well established at $0{\cdot}97 \pm 0{\cdot}02$ MeV. All that can be said with certainty is that this level is accompanied by at least one other level lying above 0·75 MeV. The second inelastic group was able to reach the counter only for scattering angles less than 90°, and corresponds to an excitation energy of $2{\cdot}15 \pm 0{\cdot}05$ MeV. There is little correlation between the excited states of aluminium-27 obtained in this way and existing information[4] on the gamma-rays which follow the beta-decay of magnesium-27.

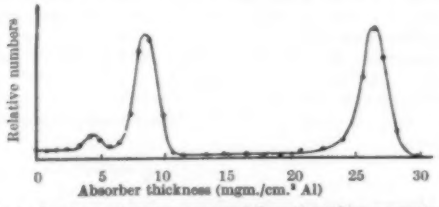

Fig. 1. Spectrum of protons scattered through 110° by magnesium (incident proton energy = 4·73 MeV.)

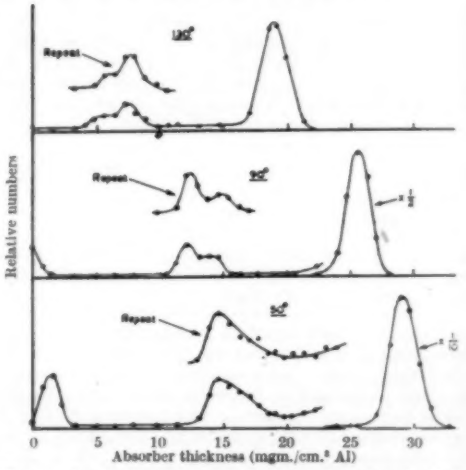

Fig. 2. Spectra of protons scattered by aluminium (incident proton energy = 4·56 MeV.). The vertical scale is not necessarily the same at all angles

The first inelastic group from magnesium is considerably larger than that from aluminium, and has a very different excitation function. The former increases sixfold on raising the incident proton energy from 4·2 to 4·7 MeV., whereas the latter is almost independent of energy in this region. This can be understood in terms of penetration of the potential barrier by the outgoing proton, since, in the case of aluminium, the proton emitted has almost enough energy to pass over the top of the barrier at the lowest bombarding energy used. This suggests that at these bombarding energies the reaction is predominantly a (p, p) process involving formation of a compound nucleus, which is interesting in view of suggestions[5,6] that nuclear excitation might be effected by Coulomb interaction. The total cross-section for excitation of magnesium to the 1·38-MeV. state by 4·73-MeV. protons is about 8×10^{-26} cm.² (assuming it to be associated with magnesium-24).

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while that for excitation of aluminium to the 0·97-MeV. state by 4·4-MeV. protons is about $1·2 \times 10^{-25}$ cm.². These figures were estimated by an indirect method, and are reliable only to within about 30 per cent.

The scattering of protons by beryllium was also investigated briefly, and an inelastic group observed corresponding to an excited state $2·39 \pm 0·05$ MeV. above the ground-state, in agreement with the results of Davis and Hafner[7]. This state is unstable with regard to neutron emission. No inelastic groups were observed from carbon, oxygen or copper.

I would like to thank Dr. A. E. Kempton and Mr. L. Coates for assistance in the experiments.

[1] Dicke and Marshall, *Phys. Rev.*, **63**, 86 (1943).
[2] Siegbahn, *Phys. Rev.*, **70**, 127 (1946).
[3] Bush and Fulbright, *Phys. Rev.*, **74**, 1206 (1948).
[4] Benes, Hedgran and Hole, *Ark. Mat. Ast. Fys.*, **35A**, Paper 12 (1948).
[5] Weisskopf, *Phys. Rev.*, **53**, 1018 (1938).
[6] Guth, *Phys. Rev.*, **69**, 280 (1945).
[7] Davis and Hafner, *Phys. Rev.*, **73**, 1473 (1948).

MOLECULAR WEIGHT DETERMINATION ON THYMONUCLEIC ACID COMPOUNDS BY DIELECTRIC MEASUREMENTS

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AND
L.-G. ALLGÉN
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Sodium Thymonucleate

THE molecular weight of a thread molecule with a permanent dipole moment such as desoxyribose nucleic acid can be calculated from the anomalous dispersion of the dielectric constant. By the so-called ellipsoid method[1] sodium thymonucleate was shown to have an extremely high dielectric increment, and the molecular weight was found to be very low (only about 16,000). This value, which was obtained only from measurements in 0·1 per cent aqueous solution, has since been found erroneous. In any event, it is quite clear that the dielectric method gives considerably lower values than those obtained by the ultracentrifuge, diffusion or streaming birefringence methods (200,000–1,500,000)[2–4].

In continuing the investigations the ellipsoid method was considerably improved and attained a considerable degree of exactness, and was especially adapted to the unexpectedly high frequencies needed for desoxyribose nucleic acid. Fig. 1 shows examples of some anomalous dispersion curves of the sodium salt of the acid (prepared according to Hammarsten[5]) at different concentrations. The arrows indicate the inflexion points (critical wave-lengths), and the broken curves are the theoretical dispersion curves according to Debye. The smaller graph shows the dielectric increments at different concentrations and at a wave-length of 1,000 metres (0·3 Mc./s.).

The new measurements with 0·1 per cent solution agree well with those previously obtained, except at the highest frequencies, where the earlier results were misleading. Nevertheless, there still occurs a small but constant deviation from the theoretical curve according to Debye (see graph). This deviation can be explained by association, but it indicates that the 'molecular weight' thus obtained is not a true one.

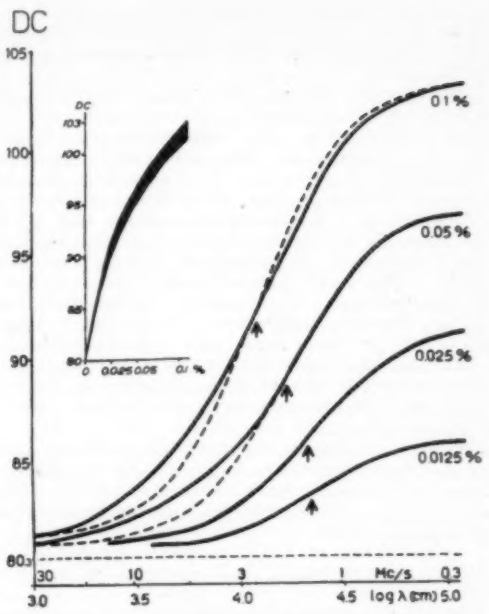

Fig. 1. Anomalous dispersion curves of sodium desoxyribose nucleate at different concentrations. Temperature 20° C. Concentrations in gm. of air-dried substance (moisture content 22·1 per cent) per 100-ml. solution

An attempt to reduce the amount of association by measuring in even more diluted solutions revealed an unexpected effect, namely, that the apparent 'molecular weight' increased with the degree of dilution. This is evident from Fig. 1, which shows that the inflexion point advances towards longer wave-lengths. The reason for this advance is not clear, but we assume that in less dilute solutions a structure is formed (by non-polar association; cf. the smaller graph). In such a structure, the larger aggregates would be impeded in their orientation in the alternating field, and the polarization would be more dependent on smaller and easily movable parts. In very dilute solutions (less than 0·03 per cent) the experimental and theoretical dispersion curves coincide, the critical wave-length is nearly constant, and a dielectrically uniform molecular weight of about 135,000 is obtained.

The high tendency to association led us to doubt whether the new value of about 135,000 was a true molecular weight. It has now also been shown that the 'molecular weight' readily changes, for example, on the addition of small quantities of salts. The effect is completely reversible. It is remarkable, however, that only insignificant additions are needed for the 'molecular weight' to fall, and at a concentration of 0·002 *M* sodium chloride we get approximately 35,000. We have not been able to get lower values. Difficulties due to the method itself prevent measurements in strong salt solutions, and other treatments of desoxyribose nucleic acid (heating,

X-ray, etc.) are not reversible. Almost the same value, 33,000, was found by Hammarsten[6] by direct osmotic measurements on sodium thymonucleate in liquid phenol.

The molecule of 35,000 has a quarter of the dipole moment of the 135,000 compound. Thus it is a pure polar association in very dilute aqueous solutions, and the aggregates are built up in exactly the same manner from a dielectric point of view as is assumed for the lower components. Obviously it is necessary to discriminate between *polymerization* and polar (probably end-to-end) *association*. In connexion with aggregates which can so easily be changed that they cannot even exist intact in solutions far below physiological salt concentration, it seems convenient to use the term *association*. *Polymerized* aggregates, on the other hand, will then be decidedly more resistant molecules. The observed value, about 35,000, is then to be regarded as the highest probable molecular weight for the sodium thymonucleate molecule.

The great tendency for desoxyribose nucleic acid to associate seems to be important for the investigation of enzymatic digestion; this should be noted, so that the easy disaggregation may not be mistaken for a depolymerization.

Nucleohistone from Calf Thymus

We have now begun investigations on desoxyribose nucleic acid nucleoproteins, and in the first place nucleohistone. Carter and Hall have made a preparation from calf thymus[7], which has been thoroughly investigated physico-chemically[8,9]. They found the nucleohistone to be monodisperse and to have a molecular weight of about 2,000,000, determined by means of measurements of sedimentation and diffusion. The axial ratio was 35.

We have made five preparations according to Carter and Hall and two others according to Stern[10]. No significant difference in respect of dielectric properties was observed. The chemical composition varied somewhat, and from the results of the analyses a desoxyribose nucleic acid content varying between 42 and 45 per cent was calculated.

Examples of some anomalous dispersion curves of nucleohistone at different concentrations are given in Fig. 2, where the inset graph shows the increments of the dielectric constant at different concentrations and a wave-length of 1,000 metres.

As appears from Fig. 2, the dispersion curves of nucleohistone exhibit the same dependence on concentration as was previously established for sodium thymonucleate (see above). We do not think that this effect can be chiefly due to a change of ion activity or solvation. We assume rather that, as in the case of desoxyribose nucleic acid, it is connected with a structural formation in more concentrated solutions, so that the individual molecule is prevented from orientating itself freely in the electric field.

Calculations of dipole moment and molecular weight were made at the lowest concentrations (less than 0·1 per cent), where the increment rises linearly with the concentration (Fig. 2, inset). The dipole moment calculated according to Debye is then 65×10^{-18} E.S.U. The molecular weight for different preparations varies between 65,000 and 80,000, with a value of about 75,000 as most probable. The content of desoxyribose nucleic acid calculated then corresponds to a molecular weight for that component of about 35,000. This value shows an interesting agreement with our presumed highest conceivable molecular weight for sodium thymonucleate and that found by Hammarsten[6]. It also indicates that the high molecular weights which will be found for desoxyribose nucleic acid in dilute aqueous solutions, and which are very easily affected by salts, are not true molecular weights.

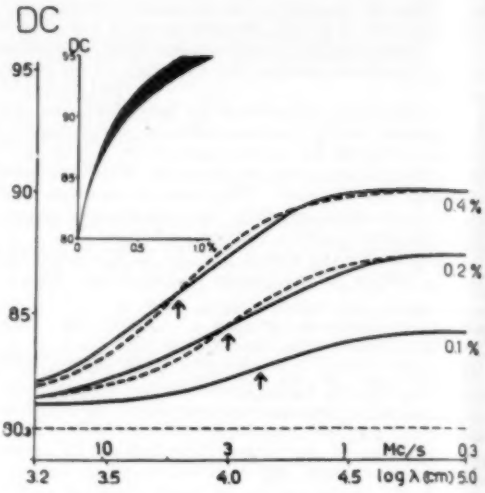

Fig. 2. Anomalous dispersion curves of nucleohistone at different concentrations. Temperature 20° C. Concentrations in gm. of air-dried substance (moisture content 11·7 per cent) per 100-ml. solution

As in the case of desoxyribose nucleic acid, the molecular weight of nucleohistone decreases considerably in salt solutions (about 25 per cent in 0·0004 *M* potassium chloride). The effect is completely reversible, also, by treatment with stronger salt solutions (for example, 1 *M* sodium chloride for several days). This decrease in molecular weight, however, differs from that caused by the influence of salt on desoxyribose nucleic acid in the appearance of a relatively high content of low molecular parts. We think, therefore, that the decrease is due to a partial dissociation of the nucleohistone, which does not necessarily influence the magnitude of the desoxyribose nucleic acid component. Unlike desoxyribose nucleic acid, the molecular weight is not changed in urea solutions.

It has recently been suggested that the nucleohistone prepared according to Carter and Hall probably suffered considerable degradation[11]. For this investigation, however, the important point is that the dielectric method gives a considerably lower value of the molecular weight for this preparation of nucleohistone, as well as for sodium desoxyribose nucleate, than that obtained by other methods.

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FULMER RESEARCH INSTITUTE

ANNUAL REPORT FOR 1947–48

THE Fulmer Research Institute, opened in July 1947, is similar in constitution to the Battelle and Mellon Institutes; its first annual report includes a brief description of the main departments—chemistry, physics and engineering, metallurgy, heat treatment—and of their more important equipment. During the eighteen months under review, the staff increased from forty-five to sixty-five, and sixteen workers with university degrees or equivalent qualifications and sponsors' nominees are now engaged in the laboratories under the director of research, Mr. E. A. C. Liddiard. The scientific staff is headed by Dr. P. Gross and Dr. A. H. Sully.

Only a general account of work in progress is given in the report. The team working under Dr. Gross has been mainly concerned with further studies of the reaction between metallic aluminium and aluminium trichloride vapour at 900–1,000° C. and at a few mm. of mercury pressure to give aluminium monochloride, AlCl. The vapour thus formed decomposes again on cooling into the trichloride and aluminium, and the reaction is expected to provide the basis of a new process for extracting and refining aluminium by 'catalytic distillation'. A semi-technical plant is under construction, and the process may possibly permit the extraction of aluminium by carbo-thermic reduction of low-grade ores. Experiments on the possibility of refining other metals by a similar technique indicate that silicon, boron and nickel can be distilled in this way.

An investigation is also in hand to develop materials with improved properties at high temperatures, particularly chromium-base alloys, and for this purpose a high-frequency generator and furnace have been constructed. Machines have also been designed and constructed in the laboratory workshops for measuring the creep of alloys in tension and in compression up to 1,000° C., and have given results of surprising accuracy. Pure alumina crucibles and other shapes have been made by slip casting and firing in the laboratories, and in view of the good service obtained, research has been extended to the refractories field. The recrystallization of refractory oxides is being studied by X-ray diffraction, and a comprehensive investigation is being made of the mechanism of precipitation in high-purity aluminium–copper alloys, with particular reference to the effects of third elements on lattice spacing. The constitution of aluminium–tin alloys has been studied with particular reference to the solid solubility of tin in aluminium and to the effects of tin on mechanical and physical properties, casting characteristics and hardening by precipitation. The properties of fluxes used in remelting aluminium cuttings and turnings are being investigated with the object of increasing their efficiency in preventing oxidation and decreasing metal losses, and a comprehensive investigation on the corrosion of aluminium alloys used in buildings is in progress. The causes and prevention of blisters and similar defects in plated aluminium articles are being studied, and various problems associated with the development of high-speed knitting machines have been solved.

Some 138 inquiries, some outside the field of light alloys or metallurgy, were dealt with in the period. A list of published notes and papers is appended, together with the names of sponsors and a list of staff.

GLASSHOUSE RESEARCH

THE annual report of the Experimental and Research Station at Cheshunt always sets forth a crop of useful results; but the latest one, which is for 1947 (the Station, Turner's Hill, Cheshunt, Herts; 1948), provides the answers to many questions on glasshouse culture. J. Littlewood and O. Owen publish the results of extensive experiments on the relation between the amounts of plant nutrients in the soil and those in the plant, with particular reference to methods of soil extraction used in advisory analyses. They conclude that extractions for potash with $N/2$ acetic acid, and for phosphoric acid with distilled water, give the most practically useful results. This is important; but even more urgent is their insistence on the determination of total nitrogen for advisory purposes, and it is impossible not to agree most strongly with this conclusion. O. Owen and G. W. Winsor have also studied in detail the question of nitrification in glasshouse soils. Farmyard manure is now scarce, and O. Owen has a useful paper on the use of sodium alginate as a substitute for it in tomato composts. The preliminary trials show that it is quite satisfactory.

The tomato moth, *Diataraxia oleracea*, causes serious injury in some commercial glasshouses. A very detailed paper by E. R. Speyer and W. J. Parr assesses its economic importance, describes the various stages of its life-history, and reviews methods for control. The pest was responsible for an annual loss of crop worth £30,000 in the Lea Valley in 1918, was later controlled to negligible proportions as a result of Lloyd's advice to spray with lead arsenate, and again assumed serious proportions following war-time changes in cropping. D.D.T. can now be used successfully against this insect; but the paper also discusses the value of other substances and methods for control. The uses of insecticides for other particular purposes are described by W. H. Read. Azobenzene fumigant and petroleum emulsions provide about equal control of red spider, and the use of hexaethyltetraphosphate is promising.

Didymella stem rot is a serious trouble. It is shown by P. H. Williams to be more severe when nitrogen is in excess in the soil, and its spores can withstand freezing temperatures. W. H. Read has tried treatment with ethyl mercury phosphate; but this does not, as yet, provide a hopeful means of control. R. Howles shows, *inter alia*, that tomato plants kept turgid by the provision of adequate water had less severe virus symptoms than when less water is given. The control of soil pathogens by means of antibiotic substances is a fascinating possibility, and E. Grossbard has found that *Penicillium patulum* will produce a growth-inhibiting substance when cultured on sugar-beet pulp or straw with glucose. The substance is antibiotic to several organisms, including *Bacillus carotovorus*, *B. phytophthorum*, *Phytophthora parasitica* and *P. cryptogea*.

More general investigations, under the control of Dr. W. F. Bewley, director of the Station, include a comparison of sterilizing by steam and by formaldehyde, the use of straw between rows, the effect of withholding lime and phosphate, the usual variety trials, and experiments on watering. These have all yielded results suitable for immediate practical application, and represent the delivery of the digested work of the Station to the industry it serves so well.

J. GRAINGER

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Tuesday, May 31

UNIVERSITY OF LONDON (in the Physiology Theatre, University College, Gower Street, London, W.C.1), at 5.15 p.m.—Prof. A. V. Hill, F.R.S.: "Muscular Contraction".* (Further Lectures on June 7 and June 14.)

INSTITUT FRANÇAIS (at Queensberry Way, London, S.W.7), at 6 p.m.—M. Paul Couderc: "Astronomie nouvelle".*

Wednesday, June 1

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 9 p.m.—Dr. Edwin H. Land: "Optical Polarisers".

Thursday, June 2

UNIVERSITY OF LONDON (in the Physiology Theatre, University College, Gower Street, London, W.C.1), at 4.45 p.m.—Dr. J. S. Bach: "Some Aspects of Nitrogen Metabolism in the Mammal".* (Further Lectures on June 9, 16 and 23.)

PHYSICAL SOCIETY (at the Science Museum, Exhibition Road, London, S.W.7), at 5 p.m.—Dr. E. H. Land: "A Colour Translating Ultra-Violet Microscope" (Twenty-sixth Duddell Lecture). (Members of the Optical Group are invited.)

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ASSISTANT PHYSICIST IN THE PHYSICS DEPARTMENT—The Secretary, Westminster Hospital, St. John's Gardens, London, S.W.1 (June 3).

LECTURER IN MATHEMATICS at the Constantine Technical College—The Director of Education, Education Offices, Woodlands Road, Middlesbrough (June 4).

LECTURER IN PHYSICS—The Clerk to the Governors, Mid-Essex Technical College and School of Art, Market Road, Chelmsford (June 4).

LECTURER IN BIOLOGY to teach botany and zoology up to Intermediate B.Sc. standard—The Principal, Aston Technical College, Whitehead Road, Birmingham 6 (June 4).

SENIOR LECTURER IN GEOGRAPHY (up to Final Degree standard with special reference to Economic and Political Geography) at the Brighton Technical College—The Education Officer, 54 Old Steine, Brighton (June 4).

LECTURERS IN SCIENCE, and a LECTURER IN MATHEMATICS, at the Royal Military Academy, Sandhurst—The Secretary, Civil Service Commission, Burlington Gardens, London, W.1, quoting No. 2564 (June 9).

SENIOR ASSISTANT IN MATHEMATICS at the Leeds College of Technology—The Director of Education, Education Department, Leeds 1 (June 11).

EDITOR of the "British Pharmaceutical Codex", "Extra Pharmacopoeia" and other scientific publications—The Secretary and Registrar, Pharmaceutical Society of Great Britain, 17 Bloomsbury Square, London, W.C.1 (June 11).

HISTOLOGIST to collaborate in researches on endocrinology of lactation and reproduction and other research projects involving histological studies—The Secretary, National Institute for Research in Dairying, Shinfield, Reading (June 11).

SENIOR SCIENTIFIC OFFICERS, SCIENTIFIC OFFICERS, EXPERIMENTAL OFFICERS and ASSISTANT EXPERIMENTAL OFFICERS, for work on the physics of metals in Ministry of Supply Research and Development Establishments in London and S. England—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.1190/48A (June 11).

PROFESSOR OF GEOLOGY, and LECTURERS IN GEOGRAPHY, BOTANY, MARINE ZOOLOGY, ORGANIC CHEMISTRY, and GEOLOGY, in the University College of the Gold Coast—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (June 13).

CHAIR OF APPLIED MATHEMATICS, THE CHAIR OF EDUCATION, THE CHAIR OF HISTORY, a LECTURER IN PHYSICS, and a LECTURER IN SOCIAL STUDIES, at Raffles College (University of Malaya), Singapore—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (June 13).

DEMONSTRATOR OF BIOLOGY—The Dean of the Medical College, St. Bartholomew's Hospital, West Smithfield, London, E.C.1 (June 15).

LECTURER (Grade II) IN APPLIED MATHEMATICS—The Secretary and Registrar, The University, Bristol (June 17).

SENIOR LECTURER or LECTURER, and an ASSISTANT LECTURER, IN THEORETICAL PHYSICS—The Registrar, The University, Manchester 13 (June 17).

PHYSICIST to the Regional Radium Institute—The Secretary, Bradford 'A' Group Hospitals Management Committee, Royal Infirmary, Bradford (June 18).

ASSISTANT MASTERS (with a degree or equivalent qualification in Mathematics, Physics, Mechanical or Electrical Engineering) in H.M. Dockyard Schools at Portsmouth, Devonport, Chatham, Sheerness and Rosyth—The Director (P.A.), Education Department, Admiralty, London, S.W.1 (June 18).

SEAGOING OBSERVERS (with a good general science background up to at least Intermediate B.Sc. standard in chemistry, physics and one other subject, preferably biology) on distant-water trawlers—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.333/49A (June 20).

DIRECTOR of the Laboratory Animals Bureau and SECRETARY of the Council's Advisory Committee on the Supply of Laboratory Animals—The Secretary, Medical Research Council, 38 Old Queen Street, London, S.W.1 (June 20).

LECTURER IN PURE MATHEMATICS—The Registrar, King's College, Strand, London, W.C.2 (June 24).

ASSISTANT LECTURERS (2) IN THE DEPARTMENT OF ORGANIC CHEMISTRY—The Registrar, The University, Liverpool (June 25).

NATURALIST at the Dove Marine Laboratory—The Registrar, King's College, Newcastle-upon-Tyne (June 25).

ASSISTANT KEEPER (Biology, with special knowledge of botany), and an ASSISTANT KEEPER (Geology, with special knowledge of palæontology)—The Director, Museum and Art Gallery, Leicester (June 25).

LECTURER or ASSISTANT LECTURER IN GEOGRAPHY—The Registrar, The University, Leeds 2 (June 25).

PLANT PHYSIOLOGIST and a PLANT BREEDER (Junior Professional Officers grade) in the Tobacco Division, Department of Agriculture, Southern Rhodesia—The Secretary to the High Commissioner for Southern Rhodesia, 429 Strand, London, W.C.2 (June 30).

KEEPER OF THE DEPARTMENT OF GEOLOGY, a KEEPER OF THE DEPARTMENT OF ETHNOLOGY, and an ASSISTANT KEEPER IN ARCHÆOLOGY AND ETHNOLOGY, in the Liverpool Museums Department—The Town Clerk, Municipal Buildings, Liverpool 2 (June 30).

PRINCIPAL SCIENTIFIC OFFICER under the Ministry of Supply for technical administration and organisation of research work in physical metallurgy carried out in academic, industrial and Government laboratories in connexion with aircraft metals and alloys—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.332/49A (June 30).

ASSISTANT KEEPER to be responsible for the Zoological Collections in the Manchester Museum—The Registrar, The University, Manchester 13 (July 1).

PRINCIPAL SCIENTIFIC OFFICER for supervision of all aspects of research and development in Helicopters from project stage to completion of tests on experimental types, under the Ministry of Supply—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting C.303/49A (July 2).

PROFESSOR OF THEORETICAL PHYSICS—The Registrar, Indian Association for the Cultivation of Science, Calcutta 12, India (July 15).

LECTURER IN MATHEMATICS, a LECTURER IN METALLURGY, and a LECTURER IN CHARGE OF GENERAL AND ADULT STUDIES, at the Wolverhampton and Staffordshire Technical College—The Clerk to the Joint Education Committee, Education Offices, North Street, Wolverhampton.

SENIOR INSPECTOR, and INSPECTORS (2), under the Seed Production Committee—The Secretary, National Institute of Agricultural Botany, Huntingdon Road, Cambridge.

LECTURERS or ASSISTANT LECTURERS (2) IN CIVIL or MECHANICAL ENGINEERING, and a LECTURER or ASSISTANT LECTURER IN METALLURGY—The Registrar, The University, Nottingham.

CARTOGRAPHERS in the War Department Map Production Establishment—The Survey Production Centre, R.E., 309 Elveden Road, London, N.W.10.

LECTURERS (2) IN THE DEPARTMENT OF MATHEMATICS—The Director of Studies, Royal Naval College, Greenwich, London, S.E.10.

LECTURER IN STATISTICS—The Registrar, The University, Reading.

METALLURGIST in the laboratory at Derby—The Scientific Research Manager, Railway Executive (London Midland Region), Euston Station, London, N.W.1.

LECTURER IN PHYSICS to Special B.Sc. standard—The Principal, West Ham Municipal College, Romford Road, Stratford, London, E.15.

LECTURER IN THE DEPARTMENT OF METALLURGY, and a GEOGRAPHY MASTER, at the County Technical College, Wednesbury—The Director of Education, County Education Offices, Stafford.

PHYSICAL CHEMIST with some research experience—The Secretary, British Rayon Research Association, 58 Whitworth Street, Manchester.

PROFESSOR OF EXPERIMENTAL PHYSICS—The Registrar, University of Manitoba, Winnipeg, Canada.

RESEARCH ASSISTANT (graduate in Physiology or Zoology) to join group investigating biological effects of radioactive substances—Dr. Janet Vaughan, Department of Pharmacology, Oxford.

MUSEUM TECHNICAL INSTRUCTOR by the Government of Nigeria—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.N.22382/3E.

LABORATORY ASSISTANT—The Headmaster, Hackney Downs School, Downs Park Road, London, E.5.

HISTOLOGICAL TECHNICIAN IN THE DEPARTMENT OF ANATOMY—The Secretary, St. Mary's Hospital Medical School, Paddington, London, W.2.

LECTURER IN BIOLOGY—The Principal, Royal Technical College, Salford 5.

MECHANICAL ENGINEER for experimental investigation of stresses in motor vehicle components, and an ELECTRICAL ENGINEER or PHYSICIST for research and development work on the application of electronic methods to mechanical engineering problems—The Director, Motor Industry Research Association, Great West Road, Brentford, Middx.

RESEARCH OFFICERS (2) for fundamental research in chemical problems connected with the manufacture of gelatine and glue—The Director, British Gelatine and Glue Research Association, Sardinia House, 52 Lincoln's Inn Fields, London, W.C.2.

ASSISTANT or ASSOCIATE PROFESSOR OF ORGANIC CHEMISTRY—The Secretary, Department of Chemistry, University of British Columbia, Vancouver, Canada.

HEAD OF THE DEPARTMENT OF PHYSIOLOGY—The Clerk to the Governors, Chelsea Polytechnic, Manresa Road, London, S.W.3.

TRA RESEARCH OFFICER in Nyasaland—The Director of Recruitment (Colonial Service), Colonial Office, Sanctuary Buildings, Great Smith Street, London, S.W.1.

TIMBER INVESTIGATIONS OFFICER for the Forestry Department, Government of Sierra Leone—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.N.22155/3E.

HONOURS GRADUATE, with first-class basic training in Chemistry or Biochemistry (or Chemical Engineering or Agricultural Chemistry), for training as a Fish Processing Expert—The Under-Secretary of State, Colonial Office (Research Department), Sanctuary Buildings, Great Smith Street, London, S.W.1.